The Long-Run Effects of Disruptive Peers

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Abstract

A large and growing literature has documented the importance of peer effects in education. However, there is relatively little evidence on the long-run educational and labor market consequences of childhood peers. We examine this question by linking administrative data on elementary school students to subsequent test scores, college attendance and completion, and earnings. To distinguish the effect of peers from confounding factors, we exploit the population variation in the proportion of children from families linked to domestic violence, who have been shown to disrupt contemporaneous behavior and learning. Results show that exposure to a disruptive peer in classes of 25 during elementary school reduces earnings at age 24 to 28 by 3 percent. We estimate that differential exposure to children linked to domestic violence explains 5 percent of the rich-poor earnings gap in our data, and that each year of exposure to a disruptive peer reduces the present discounted value of classmates' future earnings by $80,000.

1 Introduction

A large and growing literature has documented the importance of peer effects in education. This line of research has focused primarily on how peers affect contemporaneous outcomes such as test scores and disciplinary infractions in school. In contrast, relatively little is known about the long-run impact of childhood peers, particularly with respect to labor market outcomes in adulthood. This is important because it is not clear that one's peers will necessarily affect outcomes years after those peers are gone. For example, peers could primarily affect contemporaneous performance on standardized exams, rather than learning, in which case the effects could be short-lived. Similarly, while certain peers may induce some students to misbehave during school, those behavioral issues may go away when the student integrates into new and different peer groups in the future.

This lack of evidence on the long-run impacts of childhood peers has important implications for the evaluation of education policies that affect peer composition. For example, if peer effects diminish over time and do not affect adult outcomes, then concerns over how educational policies such as tracking or school vouchers affect peer composition may be overstated. On the other hand, if peers in early childhood do impact outcomes into adulthood, then it underscores the importance of concerns regarding changes in student composition. In addition, the presence of long-run peer effects also has important implications for understanding the role of sorting into schools and peer composition as determinants of income inequality. To the extent that disadvantaged groups attend schools with more disruptive peers, this differential exposure may contribute to income inequality later in life.

This paper documents the existence of long-term peer effects by estimating the effects of elementary school peers on high school test scores, college attendance and degree attainment, and earnings at age 24 to 28. It does so by linking administrative and public records data on elementary school students from a Florida county to long-term educational and earnings records. An important feature of these data is that they identify children whose families are characterized by domestic violence. This is critical for our study for two reasons. First,

exposure to domestic violence is exogenous to the student's classmates, which is critical for overcoming the reflection problem (Manski, 1993). In addition, exposure to domestic violence has been shown to be a particularly good proxy for a disruptive peer. Previous research by Carrell and Hoekstra (2010, 2012) has shown that exposure to these peers significantly disrupts contemporaneous achievement and behavior, and that these effects are driven by boys and children whose families have not yet reported the domestic violence. These contemporaneous effects are large; Carrell and Hoekstra (2010) report that having one additional classmate exposed to domestic violence reduces achievement by one-fortieth of a standard deviation, and increases disciplinary infractions by 17 percent. These findings are also consistent with a much larger literature documenting that children exposed to domestic violence are associated with a number of emotional and behavioral problems including aggressive behavior, bullying, depression, animal cruelty, diminished academic performance, and violence in adulthood (Edleson, 1999; Wolfe et al., 2003; Fantuzzo et al., 1997; Koenen et al., 2003; Holt, Buckley and Whelan, 2008; Baldry, 2003; Carlson, 2000; Currie, 2006; Black, Sussman and Unger, 2010). The purpose of this paper is to document whether exposure to these elementary school students, hereafter referred to as "disruptive" peers, affects long run educational and labor market outcomes.[1]

To distinguish the long-run effects of disruptive peers from confounding factors, we follow Hoxby (2000b) in exploiting the idiosyncratic variation in the population by including school-by-grade fixed effects.[2] Intuitively, we ask whether students never linked to domestic violence who are in cohorts with an idiosyncratically high number of disruptive peers have worse outcomes than students in the same school whose cohort had fewer disruptive peers.

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[1]In referring to these students as "disruptive", we do not mean to assume that the only mechanism through which any long-run effects arise is through classroom disruption. Rather, while we would expect much of any long-run effect to be due to classroom interaction, it could also be due to interactions separate from classroom disruptions.

[2]While Hoxby (2000b) used population variation to address the question of the impact of class size, that approach has been widely used subsequently in studying peer effects in K-12 education (Hoxby, 2000a; Lefgren, 2004; Lavy and Schlosser, 2011; Ohinata and Van Ours, 2013). In contrast, researchers examining peer effects in college have been able to identify effects using random assignment of roommates or squadrons (Sacerdote, 2001; Kremer and Levy, 2008; Carrell, Malmstrom and West, 2008; Carrell, Fullerton and West, 2009).

The identifying assumption is that all other determinants of long-run educational and labor market outcomes are orthogonal to this within-school-grade variation in peer domestic violence. Empirical evidence in this study and in previous work has shown that the within-school variation in disruptive peers is uncorrelated with cohort size and exogenous student characteristics such as own domestic violence, gender, race, and subsidized lunch status. We also show that this within-school variation in exposure to disruptive peers is uncorrelated with predicted earnings using a full set of fixed effects and covariates, which is consistent with the identifying assumption.

Results show that exposure to disruptive peers in childhood has important long-run consequences for both educational attainment as well as subsequent earnings in adulthood. Estimates indicate that exposure to one additional disruptive student in a class of 25 throughout elementary school reduces math and reading test scores in grades 9 and 10 by 0.02 standard deviations. More targeted measures of disruptive peers, such as male peers exposed to domestic violence, or peers exposed to as-yet-unreported domestic violence, result in larger effects on high school test scores and significant declines in college enrollment. Most importantly, exposure to an additional disruptive peer throughout elementary school leads to a 3 percent reduction in earnings at age 24 to 28.

Collectively, these findings demonstrate that exposure to disruptive peers in elementary school has important implications for adult outcomes. We estimate that one year of exposure to a disruptive peer in elementary school reduces the present discounted value of classmates' future earnings by around $80,000, suggesting large efficiency losses due to disruptive students. In addition, the uneven distribution of disruptive peers across schools has important consequences for income inequality. We estimate that the increased exposure to (our measure of) disruptive peers by children from lower- relative to higher-income households explains around 5 percent of the rich-poor earnings gap in adulthood.

This study's findings contribute to two different literatures. The first is a small literature that documents the persistence of peer effects on outcomes measured after the peer

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interactions. For example, Gould, Lavy and Paserman (2009) examine whether idiosyncratic cohort-to-cohort variation in exposure to immigrants during elementary school affects the passing rate on a high school matriculation exam that is necessary to attend college. They show that a 10 percentage point increase in the concentration of immigrants leads to a 2.8 percentage point decline in the passing rate. Bifulco, Fletcher and Ross (2011) report that a higher percentage of high school classmates with college-educated mothers decreases the likelihood of dropping out and increases college attendance, though Bifulco et al. (2014) show that this effect diminishes over time and that there is no evidence of an effect on labor market outcomes. Anelli and Peri (2016) analyze the long-term effects of high school gender composition and find that a higher proportion of female peers reduces the likelihood males choose a "prevalently male" major, but has no effect on graduation and labor market outcomes. Finally, Black, Devereux and Salvanes (2013) show that a higher proportion of females in ninth grade reduces mean educational attainment and the likelihood of selecting the academic (as opposed to vocational) track, but helps women by leading to lower teenage birth rates and higher earnings. They also find that higher peer father earnings leads to better outcomes, especially for men.

Our study contributes to this literature in several ways. The first is that our measure of peer quality—children from families with domestic violence—is a measure that is both exogenous to peers and also identifies students who are particularly disruptive to contemporaneous peer learning. This enables us to better measure the impact of the type of disruptive peer in the Lazear (2001) model of education. Second, because we observe test scores through the 10th grade, we are able to examine whether test score effects "fade out" over time, as has been shown to be the case in the teacher quality literature. Third, to our knowledge, we are first to identify the long-term effects of elementary school peers on adult earnings.

Finally, in assessing the long-term effects of elementary school peers on earnings, we join an emerging literature that has analyzed the long-run effects of early childhood educational inputs more generally. For example, previous studies have analyzed the long-run effects of

the Head Start and the Perry Preschool programs (Garces, Thomas and Currie, 2002; Ludwig and Miller, 2007; Heckman, Pinto and Savelyev, 2013), kindergarten classroom assignment (Krueger and Whitmore, 2001; Chetty et al., 2011; Dynarski, Hyman and Schanzenbach, 2013), and teacher value added (Chetty, Friedman and Rockoff, 2014). Our paper complements this broader literature by documenting that exposure to disruptive peers during childhood can lead to lower subsequent academic achievement in high school, a reduced likelihood of enrolling in college, and reduced earnings.

2 Data

To conduct our empirical analysis we link administrative school records to several other administrative data sets. The school records contain information on math and reading test scores (percentile rankings), as well as demographic characteristics for children attending grades 3 to 5 in the Alachua County (Florida) primary schools between the academic years 1995–1996 and 2002–2003. Alachua County is a county with a population of 218,000 (in year 2000) that is located in north-central Florida. There currently are 22 primary schools, seven middle schools, and seven high schools in the Alachua County Public Schools district, and approximately 90 percent of students attend public schools.[3] Moreover, while some elementary schools did operate gifted programs within the schools, there was no tracking at the elementary level during our time period; the first elementary gifted magnet program opened around 2003. Our dataset contains approximately 41,500 observations of 20,000 unique individuals, with around 14,000 observations per grade and a total of 10 different cohorts.

These student-level data were linked to domestic violence data that were gathered from public records information containing information on all domestic violence cases filed in civil court in Alachua County between January 1, 1993 and March 12, 2003. These cases were

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[3]This number is somewhat lower during K - 4th grade, at 86.5 percent, according to 2000 Census microdata, and increases to 94.4 percent during high school.

filed when one member of the family petitioned the court for a temporary injunction for protection against another member of the family. The data include the names and addresses of the individuals involved and the date on which the case was filed. The names and addresses are used to link the student-level information to the domestic violence data, while the date of filing is used to compute whether the domestic violence is already or yet-to-be reported at the time that the child was observed in elementary school.[4]

To link long-run education and earnings outcomes to the administrative school records from Alachua County, the data from Alachua County were sent to the Florida Department of Education (FLDOE). They linked the data to longer-term test scores as of the end of 2010, including raw test scores for grades 6 through 10. In order to have consistent test scores across grades and cohorts, we transform all the (national percentile or raw) scores into z-scores by normalizing them at the school-grade level within Alachua County, where the variance is similar to students nationwide.[5] In addition, we average the normalized math and reading scores to obtain a single score for each student. We also note that while our data do not include test scores for students who switched to private schools or moved out of state, we do observe test scores for students outside of Alachua County so long as they attended public schools within the state of Florida.

Moreover, the FLDOE provided us with information on each student's college enrollment, courses completed, and degrees attained as of the end of 2012. However, the FLDOE collects such data only for students enrolled in public post-secondary Florida institutions. To supplement these data, we collected additional college enrollment and completion data from the National Student Clearinghouse (NSC), also as of the end of 2012, which has data

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[4]For cases in which the same petitioner filed multiple requests, we used the first request.

[5]Most of our test scores—and all of our 6th - 10th grade test scores after 1999—are raw FCAT scores. While we cannot directly compare the variance of Alachua County students to students nationally on the FCAT, we note that the variance of Alachua County students is similar to Florida students, and that the variance of Florida students on the NAEP is similar to students nationally. Specifically, the ratio of Alachua standard deviation to Florida standard deviation ranges from 1.05 to 1.11 on FCAT reading scores and from 1.06 to 1.14 on FCAT math scores from 1998 - 2011. Similarly, the variance of Florida students on the NAEP is similar to U.S. students (e.g., 35 and 36 points on reading and math for 8th graders in 2005 for U.S. students compared to 36 and 37 points, respectively, for Florida students.) We thank David Figlio for graciously computing the FCAT standard deviations using statewide data.

from the majority of colleges and universities in the U.S.[6] Finally, the FLDOE also provided quarterly earnings for the students working in the state of Florida for the years 2000–2013. These earnings are transformed to 2013 real values. In order to enable us to control for age and year-by-quarter fixed effects, we then link each quarter of positive earnings between the age of 24 and 28 with each observation of a student during elementary school (up to three—the 3rd, 4th, and 5th grades). We then weight our regressions by the inverse of the number of times an individual is observed in the data.

Table 1 presents summary statistics for the main independent variables in our estimation sample, which, for reasons outlined in the following section, excludes all children who themselves were linked to domestic violence. These statistics show that around 37 percent of the sample is black and just over 50 percent are on subsidized lunch. Additionally, nearly five percent of their peers are linked to domestic violence. Roughly half of the peers linked to domestic violence are male. In addition, of the peers linked to domestic violence, around half are from homes that reported the domestic violence prior to the year and grade in which we observed them. The other half are from homes with as-yet-unreported domestic violence that was reported sometime after the year and grade in which we observed them.[7] Around 72 percent of the students in our sample have ever enrolled in college, 28 percent have received some type of college degree, and around 23 percent have received a bachelor's degree. Forty-nine percent of all individual-quarter observations are linked to positive earnings, and average quarterly earnings for those observed with positive earnings is $5,063 dollars, which is similar to reported earnings for individuals living in Alachua County of similar ages.[8,9] Our

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[6]See http://www.studentclearinghouse.org/colleges/enrollment_reporting/participating_schools.php for the full list of reporting colleges and universities.

[7]The panel nature of our elementary school data allow us to exploit the timing of the reporting of the violence. Kaci (1994) finds that on average violence had occurred in the family for over four years prior to the reporting of the incident.

[8]Source: 2013 American Community Survey and authors' calculations. We use a weighted average of earnings reported in the ACS, where weights are the proportion of earnings in our sample observed at age 24, 25, 26, 27, and 28.

[9]When analyzing college and earnings outcomes, we restrict the sample to individuals that by the end of 2012 or 2013 (last year of our education or earnings data) are old enough to be observed with the outcome of interest (age 18, 20, 22 and 24 for enrollment, any degree, college degree, and quarterly earnings respectively). Hence we use all ten cohorts for the college enrollment analysis, but only eight, six and five cohorts when

main analysis will use data only from the quarters in which individuals were observed with positive earnings. For that reason we will also explicitly test for whether being observed with positive earnings is correlated with exposure to disruptive peers during elementary school. In addition, in Section 4.5 we assess the robustness of our results to potential attrition of individuals out of Florida and to the inclusion of individuals who remain in state but have zero earnings.

3   Empirical Strategy

The two main threats to identification in the peer effects literature are the reflection and the selection problems. The reflection problem arises since it is hard to disentangle whether disruptive peers affect a student's outcomes or whether the student negatively affects her peers (Manski, 1993). To overcome this problem, we define peer quality as the proportion of one's peers whose families have been linked to domestic violence. Thus, we assume that a child's peers do not cause that child's family to be characterized by domestic violence. While we would argue that this assumption is reasonable *ex ante*, we also note that Carrell and Hoekstra (2010) explicitly test for whether own domestic violence is affected by peer domestic violence, and find no evidence of such a correlation.[10]

The selection problem arises because students self-select into schools and peer groups that are similar to them (Hoxby, 2000a). In the absence of being able to randomize students into peer groups, the main approach to overcome selection has been to exploit the natural variation in cohort composition across time within a given school (Hoxby and Weingarth, 2006; Vigdor and Nechyba, 2006; Hanushek et al., 2003; Lefgren, 2004; Bifulco, Fletcher and Ross, 2011). We follow this approach and argue that while there is selection into schools, there is natural year-to-year population variation in the proportion of peers linked

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we analyze any degree, college degree, and quarterly earnings, respectively.

[10]We also note that to the extent one believes that domestic violence is affected by one's child's classmates, one would then expect boys to be over-represented amongst families linked to domestic violence since boys have more behavioral problems. However, as noted in Table 1, boys and girls are equally likely to be linked to a family with domestic violence.

to domestic violence across cohorts within the same school. This is precisely the variation that we exploit in order to identify the impact of disruptive peers. This variation can be seen in Appendix Figure A.1, which shows the year-to-year variation in exposure to peers linked to domestic violence across cohorts. It shows substantial year-to-year variation across all schools, though schools serving lower-income populations tend to have higher concentrations of these students, as shown in Appendix Figure A.2.

We also perform an empirical test of whether this year-to-year variation at the school-grade level is consistent with a random process. Following the resampling technique used in Carrell and West (2010), for each cohort in each school and grade combination, we first randomly draw 10,000 cohorts of equal size, drawn from the relevant school/grade. Secondly, for each of the random cohorts we compute the average proportion of peers exposed to domestic violence. Thirdly, we compute empirical p-values for each of these random draws. Each empirical p-value is calculated as the proportion of simulated cohorts with a level of exposure to disruptive peers smaller than the average actually observed in that cohort. If the year-to-year variation at the school-grade level is random, we expect the distribution of the p-value to be uniform. Hence, we use a Kolmogorov-Smirnov one sample equality of distribution test to test whether the distribution of p-values is uniform, and we reject uniformity only 2 times out of 65.

We begin our analysis by focusing on a baseline model in which we control for school-by-grade fixed effects, grade-by-year fixed effects, and the proportion of peers in one's school-grade-year cohort linked to domestic violence. Specifically, we estimate the following model:

$$y_{igst} = \theta_0 + \theta_1 \frac{\sum_{k \neq i} DV_{kgst}}{n_{gst} - 1} + \theta_2 X_{igst} + \lambda_{gs} + \sigma_{gt} + \epsilon_{isgt}, \tag{1}$$

where i, g, s and t respectively represent the individual, grade, school and academic year. y represents the outcome variables of interest - test scores for grades three through ten, college enrollment, college graduation, labor force participation, and earnings.[11] Test scores

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[11]Note that these outcomes are grade invariant.

are calculated by taking the average of the reading and the math score for each student in each grade. λ and σ are grade-school and grade-year fixed effects. The coefficient of interest is θ_1, which is the coefficient on the proportion of peers from families linked to domestic violence. We note that because we exclude children who are themselves linked to domestic violence from the sample, there is no need to control for own family violence. X is a vector of additional controls that are included in some specifications. Individual-level controls include gender, race, neighborhood median family income (measured by zip code of home address), and subsidized lunch status, while cohort-level controls measure these same variables as well as both cohort size and median zip code family income at the school-grade-year level.[12] Lastly, all standard errors are clustered by the set of students who attended third through fifth grade in the same school.

In addition, because our primary goal is to assess the long-run consequences of exposure to disruptive students, we also use more targeted measures of disruptive students by focusing on certain subsets of peers shown to have especially large effects on contemporaneous outcomes. Specifically, in some specifications we focus on the impact of boys from families linked to domestic violence, since it is the boys from these families that are most disruptive to contemporaneous peer outcomes. This is also consistent with Evans, Davies and DiLillo (2008), who find that boys exposed to domestic violence are significantly more likely to exhibit externalizing behaviors. In addition, we also present specifications in which we allow children from families with as-yet-unreported domestic violence to affect their peers differently than children from families who had already reported the domestic violence. Carrell and Hoekstra (2012) show that the negative contemporaneous impact these children have on their peers abruptly disappears once the family reports the domestic violence to the court, and survey evidence suggests that reporting domestic violence helps stop the physical abuse (Kaci, 1994). As a result, we would expect that children exposed to an idiosyncratically high

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[12]Because we have relatively few cohorts—ten for test score outcomes, and five for earnings—we do not include school-specific linear time trends in our main specifications. However, those results are shown in Appendix Table A.6.

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number of peers with as-yet-unreported domestic violence will exhibit worse outcomes than children in other cohorts in that same school.

Finally, we note that because our data are composed of a panel of students who attended grades three through five in Alachua County, and because some individuals are observed with more quarters of positive earnings than others, some students are observed only once while others are observed multiple times. Consequently, all of our results are estimated using probability weights, where the weight is the inverse of the number of times a student is observed in the sample. In addition, we note that while we do not observe students while they are in the first or second grade, we expect a high level of correlation between one's peers in those grades and one's peers in grades three through five. Thus, while effects are identified using average peer exposure across the third through fifth grades, estimates are properly interpreted as the cumulative impact of disruptive peers throughout elementary school, as well as some residual exposure thereafter. We return to this issue in Section 5, when we discuss the per-year effect of exposure to disruptive peers.

Angrist (2014) raises potential concerns when estimating peer effects models. First, there is a negative mechanical correlation between own and peer characteristics when using peer averages (i.e., "leave-out-mean") as the right hand side peer variable. The solution proposed by Angrist (2014) is to use settings as in Angrist and Lang (2004) and Imberman, Kugler and Sacerdote (2012) where there is clear delineation between the individuals being affected and the individuals who are potentially affecting their peers. Similarly, we are able to break this mechanical correlation because we can clearly distinguish between students who are linked to domestic violence and those who are not. Therefore, in all of our estimates we exclude children linked to domestic violence from the data.

Second, Angrist (2014) is concerned that measurement error could lead to a positive or negative bias in peer effects estimates. To address this concern, in the spirit of Feld and Zölitz (2017), we empirically examine how adding increasing amounts of measurement error to our

data affects our point estimates.[13,14] As shown in Appendix Figure A.3, adding measurement error to the disruptive peer measure results in attenuated estimates, with larger amounts of error leading to more attenuated estimates.[15] Consequently, we conclude that our findings are unlikely to be confounded by this issue.

However, the validity of our research design could still potentially be threatened to the extent that students and families select into or out of schools on the basis of peer domestic violence. For example, our estimates could be biased if motivated parents, with higher achieving children, move their children across schools when they notice an idiosyncratically high proportion of disruptive peers in their child's grade. We note that this would be a relatively extreme response given it likely involves moving one's residence. Instead, we believe it is much more likely that certain types of parents may lobby school principals to ensure their child is not put in the same classroom as certain other children perhaps known to be disruptive, rather than moving to a new residence and school altogether. Importantly, this type of avoidance behavior within schools does not invalidate our design or bias our estimates. This is because our estimates capture the reduced-form (average) effect of treatment at the cohort level, rather than the classroom level.

Nevertheless, we perform three exercises to address the possibility of selection into and out of cohorts across schools. First, we formally test for selection by analyzing whether cohort size or other family characteristics are correlated with the proportion of peers with domestic violence. Results are shown in Table 2. In column (1), we begin by combining all of our covariates into a predicted log earnings measure for each individual, and then test whether

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[13]For each error rate (e.g., 10%), we perform the following: i) randomly create a 10% sample to which to assign error; ii) among those in the sample assigned to have error, randomly assign 4% of them (the average rate of domestic violence in our sample) to have DV=1 and the others to have DV=0; iii) create new peer variables and exclude from the sample those linked to domestic violence (a combination of actual and those mis-assigned); and iv) estimate equation (1).

[14]A previous version of the paper, NBER Working Paper 22042, included these students in the sample. Results are qualitatively similar, though estimates when excluding these students are slightly smaller. This is likely due to the fact that earnings effects are largest for those students in the left tail of the earnings distribution, as shown and discussed later. We note that when including children linked to domestic violence in the sample, estimates were insensitive to whether or not we controlled for the own domestic violence effect.

[15]We are grateful to Ulf Zolitz for suggesting this exercise.

predicted log earnings is correlated with disruptive peer exposure during elementary school. Specifically, we regress log earnings on the full set of fixed effects and controls, excluding peer domestic violence, and use the estimated coefficients to predict earnings for each individual in the sample. This measure captures a linear combination of individual characteristics, where the weights are chosen as to best predict earnings potential. As shown in column (1), this measure of earnings potential is uncorrelated with our three measures of exposure to disruptive peers.

Columns (2) through (6) show the correlation between gender, race, subsidized lunch status, and neighborhood income level. In addition, in columns (7) through (9) we show the correlation between disruptive peers and whether the student changed schools from the previous year to the current year, was observed in school without taking the test, or left the school district entirely between the third and fifth grade. Of the 40 estimates shown in columns (2) through (9), four are significant at the 10 percent level, three are significant at the 5 percent level, and none is significant at the one percent level, which is approximately what one might expect due to chance. In addition, we emphasize that the coefficients are interpreted as the effect of going from 0 to 100 percent disruptive peers, and thus need to be rescaled. These rescaled estimates are shown two rows below the estimates, and reflect the marginal effect of adding one student linked to domestic violence to a class of 25. For example, one of the largest coefficients—the marginally significant coefficient of -0.355 on unreported peer domestic violence—indicates that adding one student linked to as-yet-unreported domestic violence to a class of 25 is associated with a 1.4 (0.355/25) percentage point or 5.8 percent reduction in the likelihood of being black.[16] Our conclusion based on Table 2 is that there is little evidence to suggest that students are entering or leaving schools in a way that is systematically correlated with our three different proxies for disruptive peers.[17]

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[16]We also note that this particular correlation is the wrong sign for those concerned with selection into or out of cohorts.

[17]In Appendix Tables A.1 and A.2, we show similar tables for the subsamples of observations observed and not observed with positive earnings, respectively.

In addition, in Figure 1 we graph predicted log earnings against the percent change in residual exposure to disruptive peers (relative to the average peer exposure for that school and grade) after controlling for school-grade and grade-year fixed effects. Importantly, in this graph we do so only for individuals subsequently observed with positive adult earnings.[18] Open circles are local averages and the dashed lines are linear fits through the underlying data. Importantly, across all three measures of treatment, the relationship between predicted earnings and treatment is quite flat. That indicates there is little reason to believe that students across these different cohorts should have had different earnings levels, absent the effect of exposure to disruptive peers. In addition, because Figure 1 shows predicted earnings *only for those observed with positive adult earnings*, it demonstrates that the income-earning potential for those observed with earnings is not systematically correlated with treatment. This suggests that attrition out of the state is unlikely to bias our estimates.

Figure 1 also highlights our main findings on the long-run impact of disruptive peers on earnings. In contrast to predicted earnings, which do not vary with intensity of treatment as graphed on the x-axis, *actual* earnings (shown in solid black) do vary significantly with whether one was exposed to an idiosyncratically high or low proportion of peers linked to domestic violence. Consistent with expectations, the raw data shown in Figure 1 indicate that children who were exposed to an above-average (within-school) concentration of disruptive peers in elementary school have much lower-than-predicted earnings. Specifically, the slopes of the fitted lines for actual earnings in Figures 1a through 1c predict that adding one disruptive peer to a class of 25 will result in earnings reductions of 3.1, 3.7, and 5.2 percent, respectively.[19] Thus, while we will document the magnitude of these effects more rigorously and precisely in the next section, Figure 1 provides an illustration of both the validity of the

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[18]Tables corresponding to Table 2 for those observed and not observed with positive earnings are shown in Appendix Tables A.1 and A.2.

[19]Source: Authors' calculations. For example, a four percentage point increase in exposure represents an 87.0 percent increase over the mean of 4.6 percent. Given the slope of the line in Figure 1a is -0.00036, this implies an earnings reduction of 3.1 percent (87*0.00036). We note that these estimated declines in earnings correspond closely to the estimated effect for the same change in exposure based on the coefficients reported later in column (4) of Table 5, which are 3.3, 3.2, and 5.2 percent, respectively.

research design as well as the qualitative long run impact of peers on earnings.

4 Results

To examine the long-run consequences of exposure to disruptive peers during elementary school, we focus on three sets of outcomes. First, we examine the impact of disruptive peers on test scores during elementary school. We then ask whether the impacts of those disruptive peers are evident in middle and high school test scores, college attendance and degree attainment, and labor market earnings as adults aged 24 to 28. Importantly, for each outcome we restrict our data to the sample of students old enough to have been observed with that outcome.

In addition, we focus on three different measures of disruptive peers. The first is the proportion of peers exposed to domestic violence. We then focus on two other measures of disruptive peers previously shown to have larger impacts on contemporaneous learning: male peers from families exposed to domestic violence, and peers from families with as-yet-unreported domestic violence.

4.1 Test Scores

We begin by showing the impact of disruptive peers on contemporaneous and subsequent standardized test scores. Results are shown in Table 3, where the first two columns of Panel A assess how children linked to domestic violence affect the third- through fifth-grade test scores of their peers. The specification in column (1) includes only grade-year fixed effects and school-grade fixed effects, while column (2) additionally controls for other individual and cohort-level controls. The estimate in column (2) of -0.34 suggests that adding one disruptive student to a class of 25 reduces achievement by 0.014 of a standard deviation (1/25 * -0.34), which is shown in brackets in the second row below the coefficient.[20] Estimates in columns

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[20]We note this estimate is somewhat smaller than the corresponding estimate (scaled by standard deviation) in Carrell and Hoekstra (2010). The main reason for the difference is that excluding children exposed

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(3) and (4) indicate a more modest impact during grades 6 through 8, though the effect of that same disruptive peer during elementary school is again a reduction of around 0.01 to 0.02 standard deviations in grades 9 and 10. Only the estimates in grades 9 and 10 are statistically distinguishable from zero at the 5 percent level. Across grades, none of the estimates are statistically distinguishable from each other.

Panel B of Table 3 shows estimates of the impact of male and female peers from families linked to domestic violence. The estimate in column (2) indicates that adding one disruptive male peer to a class of 25 reduces grade 3 – 5 test scores by 0.02 standard deviations (1/25 * -0.57), while female peers from families linked to domestic violence do not appear to reduce their peers' academic performance. In short, results indicate that it is the boys from these troubled families that most negatively disrupt contemporaneous academic performance, with some evidence that these effects persist afterward into high school.

Estimates of the impact of peers exposed to as-yet-unreported and reported domestic violence are shown in Panel C of Table 3. Results indicate that it is the children from families who have not yet reported the domestic violence that negatively impact their peers' contemporaneous achievement. Estimates in columns (1) and (2) show that adding one peer with as-yet-unreported domestic violence significantly reduces test scores by between 0.03 and 0.04 standard deviations. As with the results in Panels A and B, this peer effect appears to diminish in grades 6 – 8, though it is again statistically significant and between 0.02 and 0.03 standard deviations in grades 9 – 10.

Importantly, estimates across all grade levels in Table 3 change little when including individual-level and cohort-level controls. This is consistent with the identifying assumption, and provides additional evidence that there is little evidence that high-ability students selected out of schools when they were subjected to an idiosyncratically high proportion of disruptive peers. In addition, in results shown in Appendix Table A.4 we show that students

to domestic violence from the sample, as we do in this study, reduces estimates by about 25 percent. The remaining very small difference is due to our use of raw scores in this paper, rather than percentile scores used in our previous work.

are unaffected by the proportion of peers linked to domestic violence who are one year behind them in the same school.

4.2 College Attendance and Degree Attainment

We now turn to the question of whether having disruptive peers in elementary school also leads to worsened college attendance and degree attainment. Results are shown in Table 4, which takes the same form as Table 3. Columns (1) and (2) show results for college enrollment without and with additional individual and cohort-level controls; columns (3) and (4) show results for the likelihood of receiving any college degree; and columns (5) and (6) show results for four-year degree.

Results in Table 4 indicate that elementary school exposure to boys from disruptive families and to children from families with as-yet-unreported domestic violence has significant impacts on college enrollment and degree attainment. For example, estimates in column (2) suggest that adding one disruptive boy to a class of 25 throughout elementary school leads to just over a 1 percentage point (1.4 percent) reduction in college enrollment (1/25 *-0.30), which is significant at the five percent level. Similarly, the estimate in column (2) of Panel C indicates that exposure to one peer exposed to as-yet-unreported domestic violence leads to a 1.4 percentage point (1.9 percent) reduction in college enrollment.

Estimates for degree attainment are similar, with estimates in Panels B and C indicating that exposure to a disruptive peer in a class of 25 reduces the probability of receiving any degree by 2.2 and 2.7 percentage points (8 and 9.6 percent). While these estimates are statistically significant, degree attainment was also the only outcome of the five examined for which we find effects of exposure to the cohort one year younger, as shown in Appendix Table A.4. It is difficult for us to know whether this is because one-year-younger peers do affect longer-term educational attainment, or if the correlation is spurious, or something else. Consequently, we conclude that exposure to disruptive peers during elementary school leads to lower college enrollment rates, and perhaps to lower degree attainment.

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4.3 Labor Market Outcomes

Finally, we turn to labor market outcomes. Results for the baseline specification are shown in Panel A of Table 5. Columns (1) and (2) show evidence that the proportion of peers during elementary school linked to domestic violence has little effect on labor force participation. However, there is strong evidence that these peers reduce earnings. Columns (3) and (4) show estimates for the log of quarterly earnings conditional on being observed with positive earnings. Both estimates are statistically significant at the 5 percent level. The estimates of -0.98 and -0.83 in columns (3) and (4) indicate that adding one child linked to domestic violence to a classroom of 25 reduces earnings by 3.9 and 3.3 percent, respectively. To put this in perspective, we note that while our main analysis excludes children linked to domestic violence from the sample, if we were instead to include those children in this specification we estimate that they earn 13 percent less than their peers not linked to domestic violence.

Panel B shows results for the first of our more targeted measures of disruptive peers, the focus of which is the proportion of peers who are boys and are linked to domestic violence. In columns (1) and (2) we find no evidence that exposure to these peers is correlated with the likelihood of being observed with positive earnings in the state of Florida. Estimates in columns (3) and (4) indicate that both male and female peers linked to domestic violence appear to have similar negative effects on earnings, though only two of the estimates are statistically significant at the 10 percent level, and none at the five percent level. Estimates in column (4) suggest that adding one boy disruptive peer to a class of 25 reduces peers' earnings by 3.2 percent (-0.80/25), while adding one girl from a family linked to domestic violence reduces earnings by 3.5 percent (-0.87/25).[21]

Results in Panel C of Table 5 also show strong evidence that disruptive peers, defined as those exposed to as-yet-unreported domestic violence, reduce adult earnings. While there is

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[21]We also note that the effect on earnings of an additional boy peer linked to domestic violence is much larger and more negative than the estimated effects of an additional boy generally. Results on peer gender are shown in Appendix Table A.5, which indicates that an additional boy peer to a class of 25 increases earnings by an insignificant 0.48 percent, with a 95 percent confidence interval of [-0.62 percent, 1.57 percent].

no effect of peers with unreported domestic violence on labor force participation (columns (1) and (2)), both estimates in columns (3) and (4) are statistically significant at the one percent level. These estimates indicate that adding one peer linked to as-yet-unreported domestic violence reduces earnings by 5.5 and 5.2 percent, respectively.

In summary, we find strong evidence that exposure to disruptive peers during elementary school leads to significantly lower earnings in adulthood. In addition, these effects are consistent across several measures of disruptive peers. In contrast, we find no evidence that exposure is associated with differences in labor force participation.

4.4   Subgroup Analyses and Effects across the Income Distribution

We now turn to the question of which students are most affected in the long-run by exposure to disruptive peers during elementary school. Specifically, we test for differences by gender, parental socioeconomic status (as proxied by subsidized lunch status), race, and the prevalence of families linked to domestic violence at the school level. In addition, we also examine which students along the earnings distribution are most affected.

Subgroup results are shown in Table 6. Panel A shows results for grade 9 and 10 test scores; Panel B shows estimates for college enrollment; Panel C shows estimates for graduating from college with any degree; Panel D shows results for the likelihood of being observed with positive earnings; and Panel E shows results using log earnings.

Results by gender show there are few meaningful differences between men and women with respect to the long-run impacts of disruptive peer exposure. In only one case is the estimate for men statistically different from that for women (log earnings for the proportion of peers linked to domestic violence), though even there we note that the estimates for the other two more targeted measures of disruptive peers are very similar. In fact, the only substantive difference by gender (which is not shown in Table 6 for brevity purposes) is that while disruptive boys reduce the adult earnings of both peer boys and peer girls, disruptive girls also reduce girls' adult earnings.

In the third and fourth columns of Table 6, we examine the impact of disruptive peers on the outcomes of children who come from lower- and higher-income households, measured by subsidized lunch status during elementary school. The point estimates indicate that students with higher socioeconomic status experience larger declines in their high school test scores and degree attainment, though no clear pattern emerges for earnings.[22]

The most interesting subgroup effects are shown in columns (5) and (6) of Table 6, which show that while there are relatively few differences between whites and blacks with respect to high school test scores and degree attainment, there are significant differences with respect to earnings. White students experience significant declines in earnings due to disruptive peer exposure while blacks do not; the estimate implies that exposure to one disruptive student in a class of 25 reduces earnings by 6.2 percent (-1.54/25). The corresponding estimate for blacks is a 0.1 percent reduction, which is not statistically different from zero.

The last two columns of Table 6 show results for schools with above- and below-median proportions of students linked to domestic violence. We show these estimates to test for nonlinearities in effects by assessing whether the marginal effect of disruptive peers is increasing with the proportion of disruptive peers. The results suggest little evidence of this, as estimates are similar across both sets of schools.

We also test for heterogeneous effects by class size. We categorize each school-grade-year cohort as either having above- or below-median class size for that school and grade, and interact each with our measures of disruptive peers.[23] Results are shown in Appendix Table A.7, and indicate that there is little difference in effects on test scores and earnings across class size. We note, however, that this is only suggestive, as the variation in class size we observe is potentially endogenous to cohort achievement and behavior.

Finally, we also examine whether exposure to disruptive peers affects individuals equally

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[22]We also note that by showing effects for those who were not eligible for free or reduced lunch, we are also likely excluding the vast majority of those who perhaps were linked to domestic violence but who were not matched. This is because the incidence of (observed) domestic violence is five times higher for high-income families as for low-income families in our data.

[23]For this analysis our data are limited to the 1996-99 cohorts, as those are the only cohorts for whom we could obtain class size.

across the earnings distribution. We do this in part to examine which individuals are most affected by disruptive peers, and in part to show that the average effects shown in Table 5 are not driven by earnings outliers. To do so, we estimate our main specification except that we define the dependent variable to be an indicator variable for whether the individual's quarterly earnings exceeded a given amount. We then graph the resulting estimates and 95 percent confidence intervals. Results are shown in Figure 2, where the three panels correspond to our three different measures of disruptive peers. Results are consistent in that while there are negative effects across the income distribution, the largest effects are on those individuals who earn less than $40,000 annually. That is, while exposure reduces earnings for relatively few high-earning individuals, the larger impact is to move individuals from the middle of the income distribution to the lower part of the income distribution.

In summary, results yield three patterns with respect to the heterogeneous impacts of disruptive peers. First, students seem to experience similar long-run effects across gender, socioeconomic status, and the overall prevalence of families linked to domestic violence in the school. Second, white students seem to experience larger declines in earnings due to disruptive peers relative to black students. Third, exposure to disruptive peers has the largest effects on those in the bottom half of the earnings distribution.

4.5   Robustness

One limitation of our study is that we do not observe earnings in adulthood for our entire sample. This is due in part to some individuals leaving the state of Florida, and in part to some individuals in Florida earning zero income. In this section, we examine the robustness of our results on earnings to potential non-random attrition from the sample and to the inclusion of individuals with zero earnings.

As shown earlier, we find no evidence that attrition is correlated with exposure to disruptive peers—i.e., the coefficients in the first two columns of Table 5 are not statistically significant. However, one might still worry that the statistically insignificant differential at-

trition could lead to biased estimates.[24] To assess this, we perform a bounding exercise in the spirit of Lee (2009), adapted to our setting in which we have a continuous treatment measure rather than defined treatment and control groups. To illustrate this exercise, consider that in column (2) of Panel B of Table 5 we estimate that exposure to boys linked to domestic violence is negatively correlated with being observed with positive earnings (coefficient = -0.20). We therefore drop from our sample individuals whose exposure to boys linked to domestic violence was less than the median exposure for that school and grade. More precisely, we drop enough of these individuals such that the estimate of -0.20 effectively becomes zero. We note that because our measure of exposure to disruptive peers is continuous, the number of observations that must be dropped varies depending on which individuals are dropped. As a result, we iterate the following procedure 500 times. First, we randomly and incrementally drop more individuals until the point estimate shown in column (2) of Table 5 goes to zero—i.e., is no larger than +/- 0.002. Second, we estimate and save the main treatment effect of interest. After 500 iterations, we obtain a distribution of treatment effect estimates that approximates the range of estimates we could observe under differential attrition of the magnitude estimated in Table 5.

Results from this exercise are shown in Table 7, where the first column shows our baseline estimates from column (4) of Table 5. In column (2), we show the mean coefficients from the bounding exercise, which are similar to the baseline estimates. In the third column of Table 7 we report the empirically-computed 95 percent confidence interval of the treatment effects (i.e., the 2.5th and 97.5th percentiles), in order to describe the extremes of the range of estimates. Compared to baseline estimates of 3.3, 3.3, and 5.1 percent for our three measures of disruptive peers, the upper bounds of the 95 percent confidence intervals are 3.3, 2.0, and 4.2 percent, respectively. Put differently, exposure to disruptive peers is associated with

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[24]One might also worry that even though the rate of attrition is similar across the treatment and control groups, perhaps different *types* of people in the treatment group leave the state compared to the control group. However, this is inconsistent with the results in Figure 1 and column (1) of Appendix Table A.1, which show that the earnings potential of those later observed with positive earnings—as predicted by all exogenous covariates—is uncorrelated with exposure to disruptive peers.

large reductions in earnings even under the assumption of extreme non-random attrition. As a result, we conclude that the effects we find are unlikely to be caused by moves out of state that are systematically correlated with exposure to disruptive peers.[25]

In order to examine how including individuals with zero actual earnings in the sample affects our estimates, we first estimate how many zero earners we should have in our sample. According to the American Community Survey (ACS), 70.2 percent of individuals born in Florida remain in Florida from ages 24 - 28.[26] By comparison, we observe positive earnings for 49.3 percent of the observations in our data. As a result, we estimate that 20.9 percent of the observations in our data set have missing data on earnings due to zero earnings.

We assess the potential impact of zero earners in our data by randomly assigning zeros to 20.9 percent of the observations with no positive earnings in our data, and re-estimating the results including these zeros. We again repeat this exercise 500 times in order to generate a distribution of estimates. Results are shown in Table 8. In column (1), we estimate our baseline specification using the level of earnings as the dependent variable, rather than log earnings. Consistent with the results shown in Table 5, we find strong evidence that exposure to peers linked to domestic violence reduces earnings. For example, our estimate of -2807.54 in Panel A of Table 8 indicates that exposure to one peer linked to domestic violence in a class of 25 reduces quarterly earnings by $112.28 (-2807.54/25), or 2.0 percent. We find larger effects for exposure to boys linked to domestic violence (3.4 percent) and peers linked to as-yet-unreported domestic violence (4.3 percent). Column (2) shows the average of the estimates from the 500 simulations when zero earners are included. Relative declines in earnings (given the mean of quarterly earnings) are shown in brackets. Results across all three panels indicate that while point estimates are smaller when including individuals with

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[25]We note that like the bounding exercise proposed by Lee (2009), this does not rule out the possibility of attrition by different types of "more treated" and "less treated" individuals that is offsetting in rates. We note, however, that Figure 1 suggests that the earnings propensity—as predicted by exogenous covariates—is similar across individuals with varying levels of exposure. This suggests that this type of attrition is unlikely to drive our results.

[26]In order to make this estimate as comparable as possible to our sample, we weighted each probability in the ACS with the share of earnings data we observe at those ages, which are 0.4027, 0.2948, 0.1955, 0.0862 and 0.0208 for ages 24 - 28, respectively.

zero earnings, the relative declines in earnings are similar or even larger than our baseline estimates. Specifically, we find that exposure to our three measures of disruptive peers reduces earnings by 2.1, 4.3, and 5.1 percent, respectively.

In the third column we show the 95 percent confidence interval for the estimates (i.e., the 2.5th and 97.5th percentiles) when zero earners are included. Results here indicate that even under extreme assumptions, it is unlikely that including zero earners would change our estimates meaningfully. For example, the relative earnings effects implied by the upper bounds of the 95 percent confidence intervals for our three measures of exposure to disruptive peers are -1.6, -3.6, and -4.4 percent, respectively. Finally, in columns (4)-(6) we repeat this exercise and compute median effects to examine whether outliers are driving our mean effects. Results for these median effects are qualitatively similar to the mean effects. As a result, we conclude that even under extreme assumptions as to which observations with missing earnings data represent actual zero earnings, we still find large declines in adult earnings due to exposure to disruptive peers during elementary school.

5 Discussion and Interpretation

Given the large long-run peer effects documented in the previous section, a natural question is the exact mechanism through which those effects arise. One such potential mechanism is the impact of disruptive peers on educational achievement and attainment. Our findings above indicate that exposure to an additional disruptive peer reduces ninth and tenth grade test scores by 0.016 to 0.031 standard deviations, depending on the measure of disruptive peer used. We note, however, that for the sample of students later observed with earnings, effects are somewhat larger and range from 0.024 to 0.048 standard deviations.[27] A back-of-the-envelope calculation indicates that the 9th and 10th grade test score reductions we observe

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[27]These results are shown in Appendix Table A.8. One potential reason why estimates are larger for older cohorts is because we were able to match older cohorts to more years of domestic violence records, and thus likely have less measurement error in the proportion of peers linked to domestic violence for those cohorts.

can explain around 15 percent of the total reduction in earnings.[28] Similarly, the estimated effect on the likelihood of receiving any degree explains less than 15 percent of the total effect on earnings.[29] In addition, the results of the subgroup and income distribution analyses are difficult to reconcile with the hypothesis that the effects work largely through educational achievement or attainment. For example, disruptive peers have the largest effects on the test scores of students from higher-income families, while the earnings effects are largest at the lower end of the earnings distribution. Similarly, while the effects on earnings are largest amongst whites, both whites and blacks experience similar effects on educational achievement. Finally, we also note that while effects on test scores were driven largely by exposure to boy peers linked to domestic violence, effects in Panel B of Table 6 suggest that exposure to girl peers from these families results in similar reductions in earnings.

For all of these reasons, we expect that much of the earnings effects documented above likely comes from non-cognitive skills. Unfortunately, the nature of non-cognitive skills makes it difficult to test this directly. In Appendix Table A.9, we provide some evidence by showing the impacts on suspensions during high school. Estimates for the full sample are positive, but imprecisely estimated. In addition, we estimate effects by subgroup to assess whether the heterogeneity in results for suspensions mirrors that for earnings. Results are mixed. On the one hand, the pattern of results for suspensions by family income and school-level exposure to domestic violence does not closely parallel subgroup differences in earnings effects. On the other hand, results by gender and race are consistent with a non-cognitive mechanism. Estimated effects on suspensions are similar across males and females, consistent with the

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[28]We estimate that adding one student linked to domestic violence to a classroom of 25 reduces earnings by 3.3 percent. By comparison, the same disruptive student reduces test scores by 0.024 standard deviations. Using a simple hedonic regression of log earnings on grade-by-year fixed effects, school-by-grade fixed effects, and test scores, we estimate that a one standard deviation increase in test scores is associated with a 20 percent increase in earnings. That implies a 0.024 standard deviation reduction in test scores would result in a 0.48 percent reduction in earnings, which is 15 percent of the total effect.

[29]In a survey of the literature, Belfield and Bailey (2011) report that the return to community college degrees is between 10 and 30 percent. The largest coefficient in column (4) of Table 4 is -0.67, which implies a 2.7 percentage point reduction in degree attainment, and as much as a 0.81 percent reduction in earnings (0.027*30). The corresponding earnings reduction associated with exposure to a peer linked to as-yet-unreported domestic violence from Panel C of Table 5 is 5.52 percent (-1.38/25), suggesting that degree attainment explains less than 15 percent of the total effect on earnings.

earnings results reported in Panel E of Table 6. Similarly, the results by race closely mirror the differences in earnings effects by race. Table A.9 shows large effects on suspensions for whites, and estimates close to zero for non-whites. This is similar to the effects on earnings reported in Panel E of Table 6, where all the effects were driven by whites. Consequently, we conclude that results on suspensions provide some evidence in support of non-cognitive skills as a mechanism through which disruptive peers affect earnings.

In addition, it is important to note that the likelihood the earnings effects work through non-cognitive channels is broadly consistent with the existing literature on the long-run impacts of other childhood interventions, much of which finds large long-run effects that are difficult to explain only through achievement. For example, recent studies on the Perry Preschool Program and Project Star have shown that the impact of these programs on non-cognitive skills can explain a larger share of actual earnings gains compared to their impact on cognitive performance (Almlund et al., 2011; Chetty et al., 2011; Heckman, Pinto and Savelyev, 2013). Similarly, Chetty, Friedman and Rockoff (2014) document large effects of teacher quality on earnings despite evidence that test score gains due to better teachers fade out in subsequent years, and Chetty, Hendren and Katz (2016) show that the Moving to Opportunity (MTO) treatment effects on earnings are much larger than what one would expect from increased college attainment and quality alone. Finally, the likelihood that the long-run effect of peers linked to domestic violence works through a non-cognitive channel is also consistent with recent research on peer effects in crime; Stevenson (2015) finds that the juvenile correctional center peers that increase future crime the most are those who come from difficult or dangerous homes.

It is also helpful to place the magnitudes of these effects in a larger context by comparing them to other educational inputs. We note that the estimates shown in this paper should be interpreted as cumulative effects of peer exposure during 3rd through 5th grades, as well as some exposure prior to and after that period. Using correlations between peer exposure during the 3rd through 5th grades and the earlier and later grades, we estimate that our

measure captures 5.2 cumulative years of exposure.[30] As a result, we divide those estimates by 5.2 years in order to obtain an approximate per-year estimate. Our findings indicate that one year of exposure to a disruptive boy peer reduces college enrollment by 0.2 percentage points.[31] These effects are relatively small compared to the impact of other inputs. For example, Dynarski, Hyman and Schanzenbach (2013) and Chetty et al. (2011) report that being randomly assigned to a small class rather than a regular class with 50 percent more students in Project STAR for roughly two years increased college enrollment by 2.7 and 1.8 percentage points, respectively. Garces, Thomas and Currie (2002) estimate that Head Start increased college enrollment by 9.2 percentage points, while Chetty, Friedman and Rockoff (2014) estimate that a one standard deviation increase in in teacher quality in one grade increases college attendance by 0.82 percentage points. Thus, our estimates imply that with respect to college enrollment, a year of exposure to a disruptive male peer is equivalent to a 7 to 11 percent increase in class size for one year, a 2 percent reduction in Head Start participation, or a one-fourth standard deviation reduction in teacher quality.

We can also put the magnitude of our earnings estimates in the context of existing papers on the effects of long-run educational interventions. Chetty et al. (2011) estimate that a one-standard deviation increase in overall "class quality" (which includes class size, teacher quality, peer quality, etc.) for one year results in a 9.6 percent increase in earnings. Given our estimate that one year of exposure to a disruptive peer reduces earnings by 0.7 to 1.0 percent,[32] it implies that adding one disruptive peer is equivalent to reducing overall

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[30]A simple illustration of our calculation is that if the correlation between our 3rd through 5th grade measure and the exposure in the other grades were one or zero, then we would divide our estimates by 12 or three years, respectively. We assume the same year-to-year correlation from Kindergarten (which we assume is half-time) to 2nd grade as we observe between 3rd and 5th grade, which is 0.7411. The correlation between exposure in 6th - 10th grade and 3rd - 5th grade is 0.124. We assume the same correlation between 10th and 11th grade and 11th to 12th as we observe from 9th to 10th, which is 0.3194. As a result, we compute average years of exposure as 0.5*0.4070 + 0.5492 + 0.7411 + 3*1 + 5*0.1240 + 0.0329 + 0.0105 = 5.157.

[31]Given a coefficient of -0.30 in Column (2) of Panel B in Table 4, we scale first by 1/25 to obtain the effect of cumulative elementary school exposure in a class of 25, and then divide by 5.2 to obtain the effect of each year of exposure.

[32]Coefficients in column (4) in Table 5 indicate that cumulative exposure to a disruptive peer in a class of 25 reduces earnings by 3.3 to 5.2 percent, depending on the measure of disruptive peer. Dividing these estimates by 5.2, we estimate that each year of exposure reduces earnings by 0.6 to 1.0 percent.

class quality by around 6 to 10 percent.

Similarly, Chetty, Friedman and Rockoff (2014) estimate that a one standard deviation increase in teacher quality in one grade increases earnings by 1.3 percent. Thus, our estimates of the impact of one disruptive peer for one year imply an effect that is equivalent to approximately a one-half standard deviation reduction in teacher quality. Estimates for more targeted measures of disruptive peers are larger; a year of exposure to a child linked to as-yet-unreported violence has the same effect on earnings as a 0.8 standard deviation reduction in teacher quality.

Along similar lines, we can compare our estimates to potential policy experiments. Chetty, Friedman and Rockoff (2014) estimate that replacing a teacher estimated to be in the bottom 5 percent of the distribution with an average teacher for one year would increase the present discounted value of earnings of the students in that classroom by $250,000. Under similar assumptions,[33] we estimate that one year of exposure to a disruptive student reduces the present discounted value of lifetime earnings by around $80,000.[34,35] Similarly, we estimate that exposure to a peer linked to unreported domestic violence would reduce the present discounted value of classmate earnings by $124,000. Thus, our findings imply that having three peers from families linked to domestic violence, or two peers linked to as-yet-unreported domestic violence, has roughly the same effect on peer future earnings as

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[33]First, we assume that the impact of disruptive children is constant over the life cycle using estimates from column (4) in Table 5. Second, we assume the absence of general equilibrium effects. Third, to facilitate comparison, we assume that the present discounted value of earnings for children at age 12 in our sample is the same as those in Chetty, Friedman and Rockoff (2014) at $522,000. These estimates follow Krueger (1999) in discounting earnings gains at a 3 percent real annual rate. Finally, since the earnings losses estimated here represent the impact of cumulative exposure to disruptive peers throughout elementary school, we assume that each of these effects comes from 5.2 years of exposure.

[34]This figure is based on the estimate of -0.83 in Panel A of Table 5, which suggests that one year of exposure to a disruptive peer in a class of 25 reduces earnings by 0.638 percent (1/25 *-0.83/5.2). Assuming present discounted value of earnings of $522,000 as in Chetty, Friedman and Rockoff (2014), the estimate implies that a disruptive student reduces the lifetime earnings of each of his 24 peers by $3,330, or $79,920 across all students for that year.

[35]We note that this estimate is somewhat smaller than the $100,000 figure cited in a previous version of this paper. This is because for reasons discussed earlier, we now exclude children linked to domestic violence from the data set. This reduces point estimates somewhat, as one would expect from Figure 2, which shows that disruptive peers have the largest effects on those in the left tail of the earnings distribution.

replacing an average teacher with a teacher estimated to be in the bottom 5 percent.[36] We view this as plausible; 38 percent of teachers surveyed in the 2011-12 Schools and Staffing Survey report that student misbehavior interferes with their teaching.

Our findings also have significant implications for explaining disparities in the earnings of children who grew up in low- and high-socioeconomic status households. To the extent that school and neighborhood sorting causes students from low-income families (as proxied by subsidized lunch status) to be differentially exposed to disruptive peers, that by itself may explain some of the earnings gap observed in adulthood. For example, adults who grew up in low-income households in our sample earn roughly 70 percent of what adults from higher-income households earn, though they are also exposed to roughly 50 percent more disruptive peers of the type identified in this paper. Combined with the estimates shown in Table 5, back-of-the-envelope calculations indicate that the differential exposure to disruptive peers during elementary school explains around 5 percent of the rich-poor earnings gap in adulthood.[37] We view this as a meaningful part of the earnings gap, especially since we have only one particular measure of disruptive peers.

6   Conclusion

In this paper, we document the long-run impact of disruptive peers during elementary school on subsequent standardized exam achievement, college enrollment, and earnings. To distinguish peer effects from confounding factors, we include school-by-grade fixed effects to exploit the idiosyncratic cohort-to-cohort variation in disruptive peers within schools. We proxy for

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[36]We note that it would take around five children from families linked to domestic violence to cause effects similar to that of replacing an average teacher with one who is *actually* in the bottom 5 percent. As noted in Chetty, Friedman and Rockoff (2014), because they can identify the bottom 5 percent of teachers with error, the improvement in present discounted value of earnings from replacing an estimated 5 percent teacher ($250,000) is significantly lower than the impact of replacing an actual bottom 5 percent teacher ($407,000).

[37]Source: Authors' calculations. This range comes from the estimates using log earnings or the peer domestic violence measure of disruptive peers. Estimates for the more targeted measures of disruptive peers are 3 to 5 percent. If we instead use log earnings estimates from the subgroup analyses presented in Table 6, we estimate that the increased exposure explains 21 percent, 10 percent, or 0 percent of the rich-poor earnings gap when defining a disruptive peer as any peer linked to domestic violence, a male peer linked to domestic violence, or a peer linked to as-yet-unreported domestic violence, respectively.

disruptive peers using three different measures of peers from families linked to domestic violence, who have been shown in previous work to negatively affect the contemporaneous achievement and behavior of their classmates.

Results indicate that the impact of these disruptive peers persists for years afterward and into adulthood. Estimates indicate that adding one student exposed to domestic violence to a class of 25 reduces high school test scores by 0.02 standard deviations and reduces earnings at age 24 to 28 by three percent. More targeted proxies for disruptive peers yield somewhat larger effects. These estimates reflect the impact of exposure to a disruptive peer throughout elementary school, which suggests that the per-year impact of exposure is roughly one-fifth the magnitude of these effects. These findings correspond to the same change in earnings as about a one-half standard deviation reduction in teacher quality (Chetty, Friedman and Rockoff, 2014), and imply that one year of exposure to a disruptive student reduces the present discounted value of classmates' combined total future earnings by around $80,000. We also show that due to sorting into schools, differential exposure to disruptive children explains around 5 percent of the earnings gap between those who grew up in lower-income versus higher-income families. Given that we only have one particular proxy for disruptive peers, we view this as a lower bound of the impact of disruptive elementary school peers on income inequality.

These findings illustrate the importance of peer composition in determining long-run educational attainment and labor market outcomes. This is significant, because while a large existing literature has shown that peers impact contemporaneous learning, it was unclear whether the effects persisted for years afterward. In addition, by documenting the long-term impacts of disruptive peers, our results demonstrate the importance of potential policies that could attenuate the impact of disruptive peers. While the effect of such hypothetical policies is beyond the scope of this paper, our findings suggest that the social benefits of a reasonably effective policy are likely to be substantial. Thus, just as recent findings by Chetty, Friedman and Rockoff (2014) highlight the importance of addressing teacher

quality as a way of improving long-run productivity and earnings, results here emphasize the importance of overcoming disruptive peers as a way of improving long-term outcomes.

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Figure 1: Relationship Between Disruptive Peers and Real or Predicted Log Quarterly Earnings
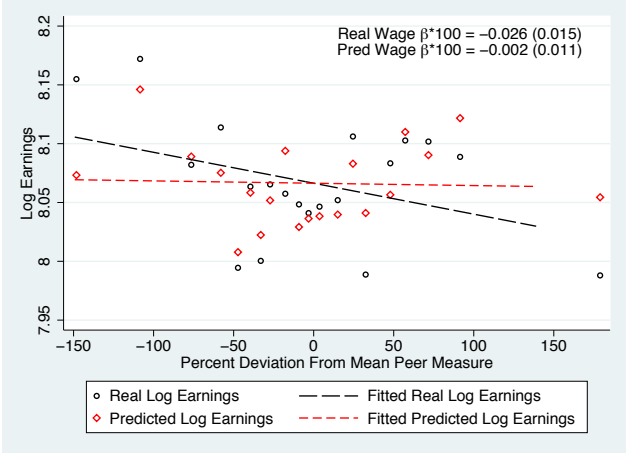
(a) Fraction Peers w/ DV

(b) Fraction Male Peers w/ DV

(c) Fraction Peers w/ Unreported DV

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. We create the predicted log earnings outcome by first running a regression that includes grade-year and school-grade fixed effects for grades third to fifth, age and quarter-by-year fixed effects, as well as additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. The regression is weighted by the inverse of the number of times a student is observed in the sample. Second, we predict log earnings using the estimated coefficients. Lastly, we collapse the data to 20 groups defined according to the percent change in residual exposure to disruptive peers (relative to the average peer exposure for that school and grade) after controlling for school-grade and grade-year fixed effects.

Figure 2: Effects of Disruptive Peers on the Distribution of Quarterly Earnings
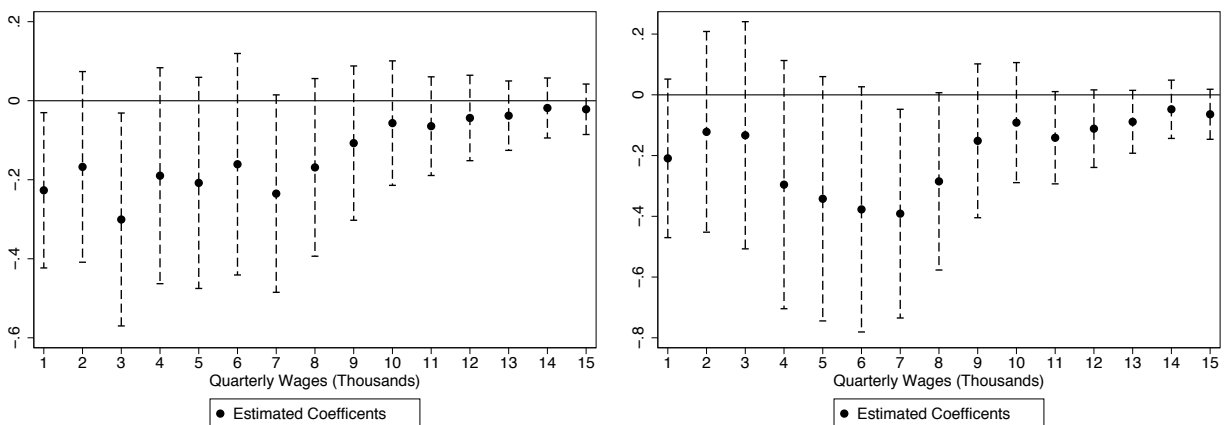
(a) Fraction Peers w/ DV

(b) Fraction Male Peers w/ DV

(c) Fraction Peers w/ Unreported DV

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each circle reports results from a separate regression, where the outcome of interest if the likelihood that an individual earns at least X thousands in quarterly earnings. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. All regressions include grade-year and school-grade fixed effects for grades third to fifth, age and quarter-by-year fixed effects, as well as individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 1: Descriptive Statistics

	Mean	Std. Dev.	Obs.
Panel A: Demographic Characteristics			
Black	0.368	(0.482)	39,573
Male	0.494	(0.500)	39,573
Free/reduced lunch	0.518	(0.500)	39,573
Fraction peers with domestic violence	0.046	(0.033)	39,573
Fraction peers with yet-to-be reported domestic violence	0.020	(0.020)	39,573
Fraction peers with already reported domestic violence	0.025	(0.023)	39,573
Fraction male peers with domestic violence	0.023	(0.022)	39,573
Fraction female peers with domestic violence	0.023	(0.020)	39,573
Panel B: Educational Attainment			
College Enrollment	0.720	(0.449)	39,573
Any Degree	0.283	(0.451)	39,054
Bacc. Degree	0.232	(0.422)	25,355
Panel C: Labor Force Outcomes - Quarterly Earnings Ages 24-28			
Positive	0.493	(0.500)	201,568
Average (Exclude Zeros) ($2013)	5,063	(9,193)	101,548

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. Sample sizes for the outcomes in Panels B and C are smaller that the full sample, as we restrict the sample to individuals that by the end of 2012 or 2013 (last year of our education or earnings data) are old enough to be observed with the outcome of interest (age 18, 20, 22 and 24 for enrollment, any degree, college degree, and quarterly earnings respectively). We restrict the sample to individuals whose family did not report domestic violence.

Table 2: Effects of Disruptive Peers on Predicted Earnings, Exogenous Student Characteristics and Attrition from Elementary School

	Predicted Log (Earnings)	Male	White	Black	Free Lunch	Median Income	Change School	Missing Score	Drop Sample
A: Exposure to Peers with DV									
Fraction Peers w/ DV	-0.196	-0.008	-0.072	-0.098	0.037	-0.083	-0.020	0.017	0.305*
	(0.398)	(0.109)	(0.106)	(0.112)	(0.097)	(0.054)	(0.056)	(0.163)	(0.168)
	{-0.008}	{-0.000}	{-0.003}	{-0.004}	{0.001}	{-0.003}	{-0.001}	{0.001}	{0.012}
B: Exposure to Male or Female Peers with DV									
Fraction Male Peers w/ DV	-0.163	0.109	-0.214	-0.023	0.195	-0.098	0.074	0.081	0.404
	(0.604)	(0.143)	(0.136)	(0.149)	(0.131)	(0.072)	(0.080)	(0.216)	(0.250)
	{-0.007}	{0.004}	{-0.009}	{-0.001}	{0.008}	{-0.004}	{0.003}	{0.003}	{0.016}
Fraction Female Peers w/ DV	-0.235	-0.129	0.074	-0.176	-0.126	-0.068	-0.117	-0.048	0.199
	(0.516)	(0.158)	(0.159)	(0.176)	(0.139)	(0.080)	(0.080)	(0.235)	(0.232)
	{-0.009}	{-0.005}	{0.003}	{-0.007}	{-0.005}	{-0.003}	{-0.005}	{-0.002}	{0.008}
C: Exposure to Peers with Unreported or Reported DV									
Fraction Peers w/ Unreported DV	-0.165	0.016	0.105	-0.355**	-0.094	0.005	0.031	0.025	0.062
	(0.520)	(0.153)	(0.154)	(0.164)	(0.145)	(0.083)	(0.079)	(0.232)	(0.279)
	{-0.007}	{0.001}	{0.004}	{-0.014}	{-0.004}	{0.000}	{0.001}	{0.001}	{0.002}
Fraction Peers w/ Reported DV	-0.231	-0.028	-0.224	0.121	0.150	-0.158**	-0.063	0.011	0.515**
	(0.610)	(0.147)	(0.145)	(0.153)	(0.150)	(0.070)	(0.078)	(0.233)	(0.242)
	{-0.009}	{-0.001}	{-0.009}	{0.005}	{0.006}	{-0.006}	{-0.003}	{0.000}	{0.021}
Mean Y	8.07	0.49	0.56	0.37	0.52	10.67	0.06	0.04	0.38
Observations	20205	39573	39573	39573	39573	39189	39573	39573	39573
Grade–Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School–Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

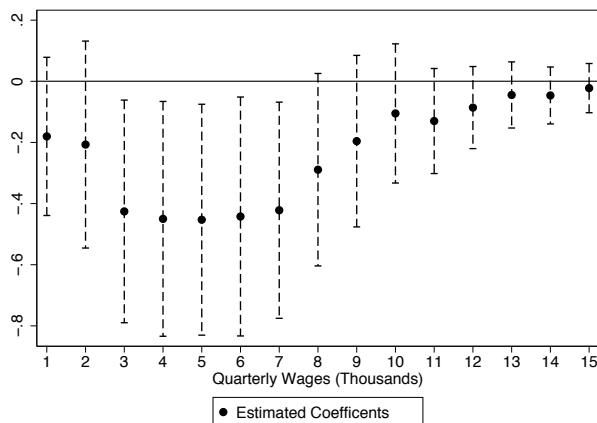Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Effects of Disruptive Peers on Test Scores

	Grades 3 to 5		Grades 6 to 8		Grades 9 and 10	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-0.36	-0.34*	-0.14	-0.11	-0.47**	-0.41**
	(0.24)	(0.20)	(0.22)	(0.18)	(0.23)	(0.18)
	{-0.014}	{-0.014}	{-0.006}	{-0.004}	{-0.019}	{-0.016}
B: Exposure to Male and Female Peers with DV						
Fraction Male Peers w/ DV	-0.70**	-0.57**	-0.38	-0.23	-0.76**	-0.59**
	(0.33)	(0.28)	(0.34)	(0.28)	(0.33)	(0.26)
	{-0.028}	{-0.023}	{-0.015}	{-0.009}	{-0.030}	{-0.024}
Fraction Female Peers w/ DV	-0.00	-0.10	0.12	0.01	-0.17	-0.22
	(0.33)	(0.27)	(0.36)	(0.29)	(0.34)	(0.25)
	{-0.000}	{-0.004}	{0.005}	{0.000}	{-0.007}	{-0.009}
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.82**	-1.04***	-0.30	-0.47*	-0.65*	-0.78***
	(0.36)	(0.29)	(0.34)	(0.26)	(0.35)	(0.27)
	{-0.033}	{-0.041}	{-0.012}	{-0.019}	{-0.026}	{-0.031}
Fraction Peers w/ Reported DV	0.04	0.27	-0.00	0.20	-0.33	-0.09
	(0.33)	(0.26)	(0.31)	(0.24)	(0.32)	(0.24)
	{0.001}	{0.011}	{-0.000}	{0.008}	{-0.013}	{-0.003}
Observations	38026	38026	36403	36403	35271	35271
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls		Yes		Yes		Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include grade-year and school-grade fixed effects for grades third to fifth. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Effects of Disruptive Peers on College Enrollment and Degree Attainment

	Enrollment		Any Degree		4-Year Degree	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-0.17	-0.15	-0.20	-0.18	-0.13	-0.09
	(0.11)	(0.11)	(0.13)	(0.13)	(0.10)	(0.11)
	{-0.007}	{-0.006 }	{-0.008}	{-0.007}	{-0.005}	{-0.004}
B: Exposure to Male and Female Peers with DV						
Fraction Male Peers w/ DV	-0.32**	-0.30**	-0.57***	-0.54***	-0.19	-0.04
	(0.15)	(0.15)	(0.17)	(0.17)	(0.15)	(0.14)
	{-0.013}	{-0.012}	{-0.023}	{-0.021}	{-0.007}	{-0.002}
Fraction Female Peers w/ DV	-0.00	0.01	0.20	0.20	-0.06	-0.14
	(0.16)	(0.15)	(0.19)	(0.19)	(0.17)	(0.15)
	{-0.000}	{0.000}	{0.008}	{0.008}	{-0.002}	{-0.006}
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.36**	-0.36**	-0.68***	-0.67***	-0.20	-0.17
	(0.16)	(0.16)	(0.18)	(0.18)	(0.15)	(0.14)
	{-0.014}	{-0.014}	{-0.027}	{-0.027}	{-0.008}	{-0.007}
Fraction Peers w/ Reported DV	-0.01	0.04	0.27	0.30*	-0.05	-0.00
	(0.16)	(0.15)	(0.18)	(0.18)	(0.14)	(0.15)
	{-0.000}	{0.002}	{0.011}	{0.012}	{-0.002}	{-0.000}
Mean Y	0.72	0.72	0.28	0.28	0.23	0.23
Observations	39573	39573	35054	35054	25355	25355
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls		Yes		Yes		Yes

Notes: Data are from the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. Each column reports results from a separate regression. We restrict the sample to individuals that by the end of 2012 (last year of our education data) are old enough to have completed the various degrees (18, 20 and 22 for enrollment, any degree and college degree, respectively). We also restrict the sample to individuals whose family did not report domestic violence. All regressions include grade-year and school-grade fixed effects for grades third to fifth. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Effects of Disruptive Peers on Labor Force Outcomes - Students Aged 24-28

	Positive Earnings		Log (Earnings)	
	(1)	(2)	(3)	(4)
A: Exposure to Peers with DV				
Fraction Peers w/ DV	-0.06	-0.00	-0.98***	-0.83**
	(0.13)	(0.13)	(0.38)	(0.38)
	{-0.002}	{-0.000}	{-0.039}	{-0.033}
B: Exposure to Male or Female Peers with DV				
Fraction Male Peers w/ DV	-0.21	-0.20	-0.94*	-0.80
	(0.20)	(0.19)	(0.57)	(0.55)
	{-0.008}	{-0.008}	{-0.038}	{-0.032}
Fraction Female Peers w/ DV	0.12	0.22	-1.03*	-0.87
	(0.19)	(0.19)	(0.54)	(0.55)
	{0.005}	{0.009}	{-0.041}	{-0.035}
C: Exposure to Peers with Unreported or Reported DV				
Fraction Peers w/ Unreported DV	-0.18	-0.15	-1.38***	-1.29***
	(0.18)	(0.17)	(0.49)	(0.50)
	{-0.007}	{-0.006}	{-0.055}	{-0.052}
Fraction Peers w/ Reported DV	0.07	0.16	-0.56	-0.34
	(0.18)	(0.18)	(0.56)	(0.55)
	{0.003}	{0.006}	{-0.022}	{-0.014}
Mean Y	0.49	0.49	8.07	8.07
Observations	201568	201568	101548	101548
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes
Additional Controls		Yes		Yes

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each column reports results from a separate regression. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. All regressions include grade-year and school-grade fixed effects for grades third to fifth, as well as age and quarter-by-year fixed effects. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Heterogeneity in the Long Term Effects of Disruptive Peers

	Gender		Income		Race		School DV - Median	
	Male	Female	Low	High	Non-White	White	Above	Below
A: Test Scores in Grades 9-10								
Fraction Peers w/ DV	-0.36	-0.47**	-0.09	-0.88***	-0.42*	-0.31	-0.53**	-0.12
	(0.28)	(0.23)	(0.24)	(0.28)	(0.24)	(0.25)	(0.24)	(0.25)
Fraction Male Peers w/ DV	-0.81**	-0.38	-0.18	-1.25***	-0.56	-0.33	-0.56*	-0.78*
	(0.37)	(0.32)	(0.30)	(0.41)	(0.34)	(0.36)	(0.32)	(0.43)
Fraction Peers w/ Unreported DV	-0.58	-1.01***	-0.25	-1.57***	-0.67*	-0.52	-0.80**	-0.40
	(0.39)	(0.34)	(0.34)	(0.39)	(0.36)	(0.39)	(0.34)	(0.40)
B: College Enrollment								
Fraction Peers w/ DV	-0.22	-0.10	-0.19	-0.23	0.05	-0.31**	-0.34**	-0.03
	(0.16)	(0.14)	(0.13)	(0.14)	(0.15)	(0.15)	(0.15)	(0.14)
Fraction Male Peers w/ DV	-0.43**	-0.19	-0.28	-0.26	-0.16	-0.36*	-0.44**	-0.23
	(0.21)	(0.19)	(0.18)	(0.22)	(0.21)	(0.20)	(0.19)	(0.25)
Fraction Peers w/ Unreported DV	-0.52**	-0.23	-0.56***	-0.28	-0.32	-0.28	-0.61***	-0.26
	(0.23)	(0.21)	(0.18)	(0.20)	(0.22)	(0.22)	(0.20)	(0.20)
C: Attainment of Any Degree								
Fraction Peers w/ DV	-0.19	-0.17	-0.07	-0.24	-0.23**	-0.02	-0.17	0.18
	(0.14)	(0.17)	(0.10)	(0.23)	(0.11)	(0.20)	(0.12)	(0.25)
Fraction Male Peers w/ DV	-0.62***	-0.49**	-0.20	-0.71**	-0.52***	-0.33	-0.21	-1.25***
	(0.21)	(0.23)	(0.13)	(0.31)	(0.17)	(0.25)	(0.15)	(0.37)
Fraction Peers w/ Unreported DV	-0.84***	-0.50*	-0.26*	-0.79***	-0.37**	-0.74***	-0.41**	-0.39
	(0.20)	(0.26)	(0.15)	(0.29)	(0.18)	(0.25)	(0.18)	(0.35)
D: Likelihood of Positive Earnings								
Fraction Peers w/ DV	0.20	-0.19	0.13	-0.15	-0.03	-0.02	-0.07	0.17
	(0.21)	(0.18)	(0.16)	(0.24)	(0.18)	(0.19)	(0.16)	(0.25)
Fraction Male Peers w/ DV	-0.22	-0.15	-0.07	-0.35	-0.22	-0.28	-0.33	0.67
	(0.28)	(0.28)	(0.22)	(0.38)	(0.26)	(0.29)	(0.21)	(0.42)
Fraction Peers w/ Unreported DV	-0.03	-0.26	0.17	-0.59*	-0.15	-0.22	-0.29	0.22
	(0.27)	(0.23)	(0.22)	(0.32)	(0.25)	(0.25)	(0.20)	(0.32)
E: Log (Earnings)								
Fraction Peers w/ DV	-0.25	-1.42***	-1.17***	-0.40	-0.24	-1.54***	-0.98**	-0.55
	(0.51)	(0.52)	(0.45)	(0.63)	(0.41)	(0.55)	(0.44)	(0.54)
Fraction Male Peers w/ DV	-0.70	-0.83	-1.01*	-0.56	-0.60	-1.26	-0.86	-0.98
	(0.87)	(0.67)	(0.58)	(0.97)	(0.54)	(0.82)	(0.60)	(0.72)
Fraction Peers w/ Unreported DV	-1.28*	-1.20*	-0.78	-2.04**	-0.00	-2.55***	-1.25**	-1.29
	(0.72)	(0.67)	(0.56)	(0.84)	(0.63)	(0.73)	(0.57)	(0.81)

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. Each column and raw reports results from a separate regression. Sample sizes vary by outcome analyzed, as we restrict the sample to individuals that by the end of 2012 or 2013 (last year of our education or earnings data) are old enough to be observed with the outcome of interest (age 18, 20, 22 and 24 for enrollment, any degree, college degree, and quarterly earnings respectively). We also restrict the sample to individuals whose family did not report domestic violence. All regressions include controls for individual and cohort level controls, as well as grade-year and school-grade fixed effects for grades third to fifth. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Regressions for earnings outcomes also include age and quarter-by-year fixed effects. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Effect of DisruptivePeers on Log Earnings – Randomly Drop "Extra" Observations

	Baseline	Randomly Drop Observations	
	Log Earnings	Average	95% CI
Fraction Peers w/ DV	-0.834**	-0.834	[-0.858,-0.813]
	(0.377)		
Fraction Male Peers w/ DV	-0.832	-0.791	[-1.134,-0.502]
	(0.544)		
Fraction Female Peers w/ DV	-0.898	-0.893	[-1.074,-0.727]
	(0.572)		
Fraction Peers w/ Unreported DV	-1.279**	-1.251	[-1.438,-1.050]
	(0.502)		
Fraction Peers w/ Reported DV	-0.285	-0.294	[-0.475,-0.089]
	(0.541)		
Number of Random Draws		500	
Grade-Year FEs (Grades 3-5)	Yes	Yes	
School-Grade FEs (Grades 3-5)	Yes	Yes	
Additional Controls	Yes	Yes	

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. The first column presents our baseline coefficients on the effect of disruptive peers on log quarterly earnings, as well as their standard errors (in parentheses). The second and third columns presents average estimated coefficients as well as they 95% range in our bounding exercise, where we randomly and for 500 times drop "extra" observations and estimate the effect of disruptive peers on log quarterly earnings. All regressions include grade-year and school-grade fixed effects for grades third to fifth, age and quarter-by-year fixed effects, as well as additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table 8: Effect of Disruptive Peers on Earnings – Include "True" Zeros

	Average Effects			Median Effects		
	Earnings	Include True Zeros		Earnings	Include True Zeros	
	Level	Average	95% CI	Level	Average	95% CI
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-2807.54*	-2085.34	[-2577.93, -1617.32]	-948.40	-1287.52	[-2062.13, -516.19]
	(1678.89)			(607.37)		
	{-0.020}	{-0.021}	{-0.026, -0.016}	{-0.007}	{-0.013}	{-0.021, -0.005}
B: Exposure to Male or Female Peers with DV						
Fraction Male Peers w/ DV	-4790.46**	-4230.01	[-4840.87, -3611.35]	-3046.77***	-2923.92	[-4114.75, -1823.849]
	(2324.17)			(926.06)		
	{-0.034}	{-0.043}	{-0.049, -0.036}	{-0.021}	{-0.029}	{-0.041, -0.018}
Fraction Female Peers w/ DV	-493.22	342.70	[-399.53, 1080.86]	1429.34	613.98	[-600.81, 1768.62]
	(1970.86)			(948.19)		
	{-0.003}	{0.003}	{-0.004, 0.011}	{0.010}	{0.006}	{-0.006, -0.018}
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-6068.34**	-5059.64	[-5648.35, -4360.29]	-2116.31**	-4067.23	[-5071.99, -2976.31]
	(2826.45)			(904.72)		
	{-0.043}	{-0.051}	{-0.057, -0.044}	{-0.015}	{-0.041}	{-0.051, -0.030}
Fraction Peers w/ Reported DV	670.67	1077.02	[416.87, 1767.06]	274.10	1701.86	[591.26, 2690.94]
	(1910.47)			(897.02)		
	{0.005}	{0.011}	{0.004, 0.018}	{0.002}	{0.017}	{0.006, 0.027}
Mean Y	5686.30		3969.40	5686.30		3969.40
Number of Random Draws			500			500
Observations	101548		143675	101548		143675
Grade-Year FEs (Grades 3-5)	Yes		Yes	Yes		Yes
School-Grade FEs (Grades 3-5)	Yes		Yes	Yes		Yes
Additional Controls	Yes		Yes	Yes		Yes

Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data) and whose family did not report domestic violence. The first column presents our baseline coefficients on the effect of disruptive peers on quarterly earnings, as well as their standard errors (in parentheses). The second and third columns presents average estimated coefficients as well as they 95% range in the exercise where we randomly include for 500 times "true" observations with zero earnings and estimate the effect of disruptive peers on quarterly earnings. Columns (4) to (6) contain similar results for median instead of average effects. All regressions include grade-year and school-grade fixed effects for grades third to fifth, age and quarter-by-year fixed effects, as well as additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is possibly observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

A   Appendix

Figure A.1: Average Fraction of Disruptive Peers By School and Cohort
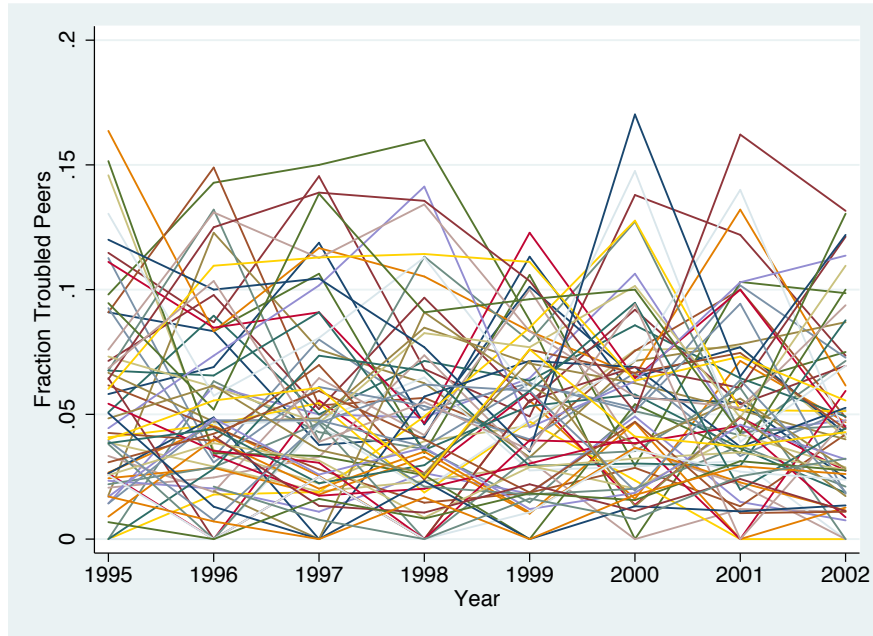
Notes: Data are from the Alachua County School District and the Alachua County Court-house. Each line represents the average fraction of disruptive peers in each Alachua county school and grade and shows how this fraction changes over time.

Figure A.2: Relationship Between Fraction Children on Free/Reduced Lunch
and Fraction of Disruptive Peers
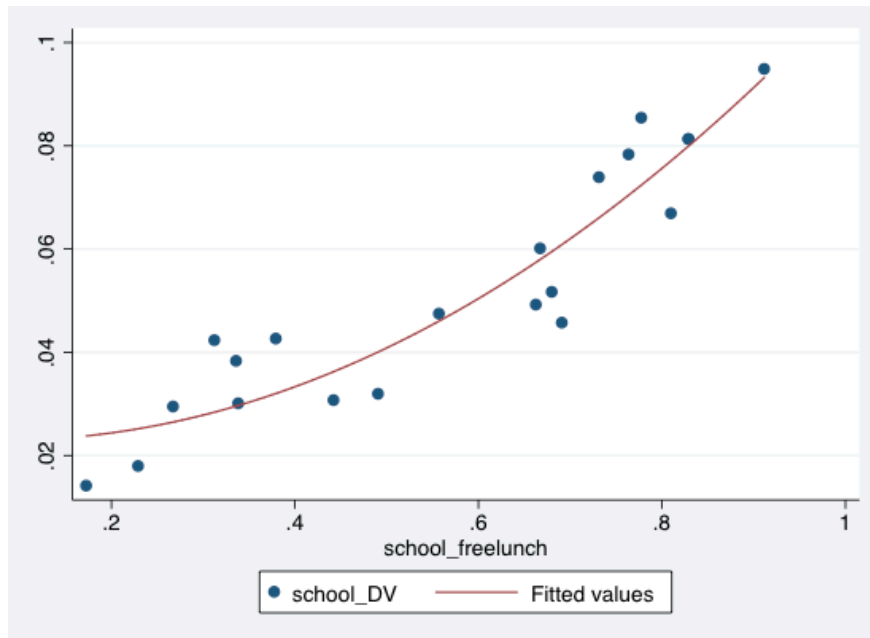
Notes: Data were first collapsed into average disruptive peer exposure at the school-grade
level. Each point represents a local average for the set of school-grades for which the
average fraction of students on free/reduced lunch is within a given range. Each point
represents an equal number of school-grades.

Figure A.3: Effects of Disruptive Peers on Log Wages – Sensitivity to Measurement Error in the Domestic Violence Variable
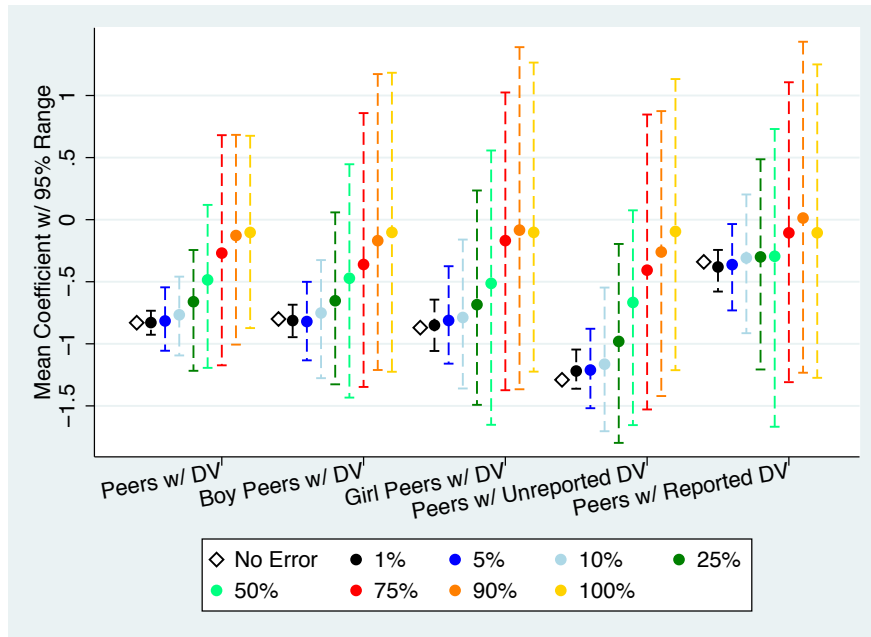
Notes: Data are from the Florida Department of Education (FDOE) and the Alachua County Courthouse. Each scatter point represents the average estimated coefficient (and 95% range) obtained when introducing measurement error in the domestic violence variable in 1, 5, 10, 25, 50, 75, 90 and 100 percent of the sample. All regressions include controls for grade-year and school-grade fixed effects for grades third to fifth, age and quarter-by-year fixed effects, as well as additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. In all regressions we restrict the sample to individuals whose family did not report domestic violence, and we weight by the inverse of the number of times a student is observed in the sample.

Table A.1: Effects of Disruptive Peers on Predicted Earnings and Exogenous Outcomes –
Positive Quarterly Wage Observations

	Predicted Log (Earnings)	Male	White	Black	Free Lunch	Median Income
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-0.159	0.135	-0.241	0.253	0.389**	-0.059
	(0.445)	(0.176)	(0.181)	(0.184)	(0.195)	(0.093)
	{-0.006}	{0.005}	{-0.010}	{0.010}	{0.016}	{-0.002}
B: Exposure to Male or Female Peers with DV						
Fraction Male Peers w/ DV	-0.040	-0.065	-0.517**	0.371	0.639**	-0.063
	(0.682)	(0.273)	(0.257)	(0.261)	(0.269)	(0.129)
	{-0.002}	{-0.003}	{-0.021}	{0.015}	{0.026}	{-0.003}
Fraction Female Peers w/ DV	-0.300	0.373	0.087	0.113	0.092	-0.055
	(0.581)	(0.387)	(0.263)	(0.294)	(0.254)	(0.168)
	{-0.012}	{0.015}	{0.003}	{0.005}	{0.004}	{-0.002}
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.070	0.132	-0.373	0.256	0.804***	-0.173
	(0.588)	(0.247)	(0.232)	(0.219)	(0.285)	(0.129)
	{-0.003}	{0.005}	{-0.015}	{0.010}	{0.032}	{-0.007}
Fraction Peers w/ Reported DV	-0.254	0.138	-0.099	0.250	-0.056	0.062
	(0.663)	(0.294)	(0.275)	(0.289)	(0.253)	(0.123)
	{-0.010}	{0.006}	{-0.004}	{0.010}	{-0.002}	{0.002}
Mean Y	8.07	0.47	0.60	0.35	0.49	10.66
Observations	101548	101548	101548	101548	101548	100221
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data), have positive quarterly wage earnings, and whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

Table A.2: Effects of Disruptive Peers on on Predicted Earnings and Exogenous Outcomes –
Zero Quarterly Wage Observations

	Predicted Log (Earnings)	Male	White	Black	Free Lunch	Median Income
A: Exposure to Peers with DV						
Fraction Peers w/ DV	-0.036	-0.106	-0.158	-0.076	0.054	-0.100
	(0.430)	(0.217)	(0.240)	(0.232)	(0.173)	(0.111)
	{-0.001}	{-0.004}	{-0.006}	{-0.003}	{0.002}	{-0.004}
B: Exposure to Male or Female Peers with DV						
Fraction Male Peers w/ DV	-0.192	0.188	-0.487	0.279	0.433*	-0.079
	(0.642)	(0.294)	(0.302)	(0.302)	(0.234)	(0.154)
	{-0.008}	{0.008}	{-0.019}	{0.011}	{0.017}	{-0.003}
Fraction Female Peers w/ DV	0.141	-0.438	0.213	-0.477	-0.372	-0.125
	(0.575)	(0.292)	(0.353)	(0.354)	(0.293)	(0.174)
	{0.006}	{-0.018}	{0.009}	{-0.019}	{-0.015}	{-0.005}
C: Exposure to Peers with Unreported or Reported DV						
Fraction Peers w/ Unreported DV	-0.042	-0.005	-0.129	-0.011	-0.048	0.016
	(0.565)	(0.280)	(0.331)	(0.308)	(0.239)	(0.142)
	{-0.002}	{-0.000}	{-0.005}	{-0.000}	{-0.002}	{0.001}
Fraction Peers w/ Reported DV	-0.029	-0.217	-0.190	-0.149	0.167	-0.229
	(0.655)	(0.328)	(0.349)	(0.352)	(0.263)	(0.149)
	{-0.001}	{-0.009}	{-0.008}	{-0.006}	{0.007}	{-0.009}
Mean Y	8.07	0.51	0.60	0.32	0.51	10.67
Observations	100020	100020	100020	100020	100020	98839
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals that are at least 24 years old by 2013 (last year of our earnings data), have zero quarterly wage earnings, and whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. The marginal effect of adding one disruptive peer to a class of 25 is shown in brackets, and is defined as the coefficient divided by 25. * p<0.10, ** p<0.05, *** p<0.01.

Table A.3: Effects of Disruptive Peers on Test Scores for Each Grade

	Average Score in Grade:							
	3rd	4th	5th	6th	7th	8th	9th	10th
Fraction Peers w/ DV	-0.20	-0.32	-0.67***	0.09	-0.28	-0.20	-0.42**	-0.52***
	(0.28)	(0.21)	(0.23)	(0.24)	(0.23)	(0.20)	(0.19)	(0.18)
Observations	28876	32473	28429	27407	31353	34638	34000	32769

Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Effects of Disruptive Peers in Own and One-Year-Younger Cohorts on Main Outcomes

	Grades 9 and 10		Enrollment		Any Degree		Positive Earnings		Log (Earnings)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Exposure to Peers with DV										
Fraction Peers w/ DV	-0.35*		-0.21*		-0.19		-0.00		-0.83**	
	(0.18)		(0.12)		(0.13)		(0.13)		(0.38)	
Younger Peers w/ DV		0.01		-0.07		-0.21*		0.13		-0.01
		(0.20)		(0.11)		(0.12)		(0.14)		(0.35)
B: Exposure to Male and Female Peers with DV										
Fraction Male Peers w/ DV	-0.67***		-0.32**		-0.45***		-0.20		-0.80	
	(0.26)		(0.16)		(0.17)		(0.19)		(0.55)	
Fraction Female Peers w/ DV	0.00		-0.11		0.09		0.22		-0.87	
	(0.27)		(0.17)		(0.19)		(0.19)		(0.55)	
Younger Male Peers w/ DV		0.44		-0.05		-0.49***		0.05		-0.21
		(0.28)		(0.15)		(0.17)		(0.19)		(0.47)
Younger Female Peers w/ DV		-0.45		-0.10		0.11		0.23		0.24
		(0.28)		(0.15)		(0.19)		(0.25)		(0.53)
C: Exposure to Peers with Unreported or Reported DV										
Fraction Peers w/ Unreported DV	-0.61**		-0.42***		-0.54***		-0.15		-1.29***	
	(0.26)		(0.16)		(0.17)		(0.17)		(0.50)	
Fraction Peers w/ Reported DV	-0.09		-0.02		0.16		0.16		-0.34	
	(0.24)		(0.16)		(0.18)		(0.18)		(0.55)	
Younger Peers w/ Unreported DV		-0.20		-0.25		-0.56***		0.04		-0.30
		(0.31)		(0.17)		(0.19)		(0.19)		(0.48)
Younger Peers w/ Reported DV		0.17		0.07		0.08		0.22		0.28
		(0.25)		(0.16)		(0.16)		(0.19)		(0.47)
Observations	31246	31246	35065	35065	33525	33525	201568	201568	101548	101548
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Regressions for earnings outcomes also include age and quarter-by-year fixed effects. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Effects of Male Peers on Main Outcomes

	Grades 9 and 10		Enrollment		Any Degree		Positive Earnings		Log (Earnings)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fraction Male Peers	0.12	0.05	0.04	0.01	0.01	-0.00	0.10*	0.08	0.10	0.12
	(0.10)	(0.07)	(0.05)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)	(0.14)	(0.14)

Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the National Student Clearinghouse (NSC). We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include individual controls, cohort controls and grade-year and school-grade fixed effects for grades third to fifth. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Regressions for earnings outcomes also include age and quarter-by-year fixed effects. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Effects of Disruptive Peers on Main Outcomes - Include Linear Trends

	Test Scores	College		Earnings	
	Grades 9-10	Enrolled	Any Degree	Positive	Log
A: Exposure to Peers with DV					
Fraction Peers w/ DV	-0.18	-0.19*	-0.07	-0.06	-0.80*
	(0.19)	(0.11)	(0.10)	(0.15)	(0.45)
B: Exposure to Male and Female Peers with DV					
Fraction Male Peers w/ DV	-0.38	-0.26*	-0.29**	-0.39**	-1.06*
	(0.27)	(0.15)	(0.14)	(0.19)	(0.63)
Fraction Female Peers w/ DV	0.04	-0.13	0.17	0.39	-0.44
	(0.26)	(0.16)	(0.15)	(0.25)	(0.71)
C: Exposure to Peers with Unreported or Reported DV					
Fraction Peers w/ Unreported DV	-0.36	-0.31*	-0.36**	-0.19	-1.08*
	(0.28)	(0.16)	(0.15)	(0.20)	(0.65)
Fraction Peers w/ Reported DV	-0.03	-0.10	0.19	0.05	-0.56
	(0.24)	(0.14)	(0.16)	(0.19)	(0.57)
Observations	35271	39573	35054	201568	101548
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes
School Linear Trends	Yes	Yes	Yes	Yes	Yes

Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include individual controls, cohort controls, grade-year and school-grade fixed effects for grades third to fifth, and school linear time trends. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Regressions for earnings outcomes also include age and quarter-by-year fixed effects. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Effects of Disruptive Peers on Main Outcomes - Differential Effects by Class Size

	Test Scores	College		Earnings	
	Grades 9-10	Enrolled	Any Degree	Positive	Log
A: Exposure to Peers with DV					
Class Size Below Median * Fraction Peers w/ DV	-0.50*	-0.07	-0.17	-0.10	-0.89**
	(0.26)	(0.16)	(0.15)	(0.14)	(0.42)
Class Size Above Median * Fraction Peers w/ DV	-0.48	-0.59***	-0.57***	0.25	-0.99*
	(0.32)	(0.22)	(0.18)	(0.21)	(0.58)
Class Size Below Median	0.01	-0.01	-0.01	0.03**	-0.02
	(0.02)	(0.01)	(0.01)	(0.01)	(0.04)
B: Exposure to Male and Female Peers with DV					
Class Size Below Median * Fraction Male Peers w/ DV	-0.65*	-0.19	0.00	-0.42*	-1.00
	(0.36)	(0.26)	(0.19)	(0.22)	(0.74)
Class Size Above Median * Fraction Male Peers w/ DV	-1.13**	-0.92***	-0.85***	-0.18	-1.53*
	(0.50)	(0.34)	(0.28)	(0.29)	(0.80)
Class Size Below Median	-0.00	-0.00	-0.01	0.02	-0.02
	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)
C: Exposure to Peers with Unreported or Reported DV					
Class Size Below Median * Fraction Peers w/ Unreported DV	-0.49	0.07	-0.02	-0.53**	-1.54**
	(0.43)	(0.24)	(0.20)	(0.23)	(0.68)
Class Size Above Median * Fraction Peers w/ Unreported DV	-0.55	-0.84***	-0.67***	0.18	-1.60**
	(0.40)	(0.28)	(0.24)	(0.25)	(0.71)
Class Size Below Median	0.01	-0.01	-0.01	0.03**	-0.02
	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)
Observations	18007	20287	20287	194732	97980
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes

Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include individual controls, cohort controls, grade-year and school-grade fixed effects for grades third to fifth, and school linear time trends. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. Regressions for earnings outcomes also include age and quarter-by-year fixed effects. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.8: Effects of Disruptive Peers on Test Scores and College Outcomes - Individuals At Least 24 by 2013

	At Least 24 by 2013						Observed At Least Once with Positive Earnings During Ages 24-28					
	Test Scores in Grades			College	Degree		Test Scores in Grades			College	Degree	
	3-5	6-8	9-10	Enroll	Any	4-Year	3-5	6-8	9-10	Enroll	Any	4-Year
A: Exposure to Peers with DV												
Fraction Peers w/ DV	-0.81***	-0.48**	-0.61**	-0.39**	-0.31**	-0.10	-1.11***	-0.92***	-0.68**	-0.28**	-0.45***	-0.07
	(0.27)	(0.24)	(0.25)	(0.15)	(0.13)	(0.12)	(0.32)	(0.26)	(0.26)	(0.14)	(0.16)	(0.13)
B: Exposure to Male and Female Peers with DV												
Fraction Male Peers w/ DV	-1.31***	-0.65	-1.21***	-0.69***	-0.37**	-0.15	-1.47***	-1.27***	-1.16***	-0.60***	-0.39*	-0.10
	(0.36)	(0.40)	(0.36)	(0.21)	(0.17)	(0.15)	(0.42)	(0.40)	(0.37)	(0.19)	(0.20)	(0.17)
Fraction Female Peers w/ DV	-0.25	-0.28	0.06	-0.05	-0.25	-0.03	-0.68	-0.52	-0.11	0.09	-0.52**	-0.04
	(0.37)	(0.44)	(0.36)	(0.23)	(0.19)	(0.16)	(0.47)	(0.46)	(0.37)	(0.21)	(0.23)	(0.19)
C: Exposure to Peers with Unreported or Reported DV												
Fraction Peers w/ Unreported DV	-1.43***	-0.94***	-0.97***	-0.56***	-0.49***	-0.18	-1.88***	-1.25***	-1.02***	-0.46**	-0.61***	-0.19
	(0.39)	(0.34)	(0.35)	(0.20)	(0.18)	(0.16)	(0.43)	(0.36)	(0.36)	(0.19)	(0.19)	(0.17)
Fraction Peers w/ Reported DV	-0.13	0.01	-0.24	-0.21	-0.12	-0.00	-0.27	-0.58	-0.31	-0.09	-0.27	0.05
	(0.39)	(0.34)	(0.32)	(0.23)	(0.20)	(0.17)	(0.45)	(0.37)	(0.36)	(0.22)	(0.26)	(0.18)
Observations	18675	18250	17994	20205	20205	20205	12646	12952	13001	13661	13661	13661
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the Alachua County School District, the Florida Department of Education (FDOE), the National Student Clearinghouse (NSC), and the Alachua County Courthouse. Each column and raw reports results from a separate regression. We restrict the sample to individuals at least 24 years old by 2013. We also restrict the sample to individuals whose family did not report domestic violence. All regressions include controls for individual and cohort level controls, as well as grade-year and school-grade fixed effects for grades third to fifth. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.

Table A.9: Effects of Disruptive Peers on Number of Suspensions

	All	Gender		Income		Race		School DV - Median	
		Male	Female	Low	High	Non-White	White	Above	Below
A: Exposure to Peers with DV									
Fraction Peers w/ DV	0.22	0.23	0.30	0.07	0.53**	-0.22	0.62**	-0.17	0.70*
	(0.32)	(0.51)	(0.31)	(0.45)	(0.25)	(0.49)	(0.31)	(0.41)	(0.37)
B: Exposure to Male and Female Peers with DV									
Fraction Male Peers w/ DV	0.29	0.42	0.18	0.23	0.43	0.07	0.53	0.03	0.80
	(0.44)	(0.70)	(0.42)	(0.57)	(0.37)	(0.66)	(0.43)	(0.51)	(0.58)
Fraction Female Peers w/ DV	0.14	0.04	0.43	-0.12	0.61*	-0.55	0.70	-0.41	0.62
	(0.42)	(0.70)	(0.42)	(0.65)	(0.34)	(0.68)	(0.44)	(0.62)	(0.51)
C: Exposure to Peers with Unreported or Reported DV									
Proportion Peers w/ Unreported DV	0.48	0.60	0.57	0.23	0.90**	0.25	0.92**	-0.18	0.78
	(0.47)	(0.83)	(0.41)	(0.64)	(0.43)	(0.72)	(0.43)	(0.59)	(0.54)
Proportion Peers w/ Reported DV	-0.01	-0.08	0.06	-0.08	0.23	-0.63	0.37	-0.16	0.65
	(0.41)	(0.65)	(0.42)	(0.61)	(0.31)	(0.63)	(0.44)	(0.57)	(0.49)
Mean Y	0.458	0.601	0.318	0.714	0.172	0.706	0.260	0.659	0.324
Observations	36363	17878	18485	18981	17382	16334	20029	14512	21851
Grade-Year FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Grade FEs (Grades 3-5)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Data are from the Alachua County School District, the Florida Department of Education (FDOE), and the Alachua County Courthouse. We restrict the sample to individuals whose family did not report domestic violence. Each column reports results from a separate regression. All regressions include grade-year and school-grade fixed effects for grades third to fifth. Regressions in the even numbered columns include additional individual and cohort level controls. Individual controls include gender, race, median family income, and subsidized lunch status. Cohort controls include average gender, race, subsidized lunch, and size of cohort by school-by-grade-by-year. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors, shown in parentheses, are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01.